The American Sociologist

Volume 15 Number 1 February 1980
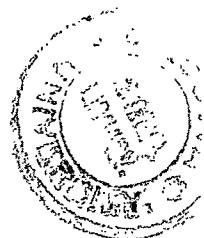
official journal of the American Sociological Association

Inviting papers for a special issue exposes an editor to several hazards. What if no one shares the editor's sense of urgency about the topic? What if the thinking and writing about the topic are so diffuse that most of the submitted papers are poorly conceived?

In presenting this special issue on constraints and scenarios for sociology curricula, I feel that we have survived most of the hazards. Judging from the large volume of manuscripts that were submitted, clearly many persons are deeply concerned about constraints and scenarios for undergraduate curricula. Also, they write articulately about their concerns.

The reviewers were enthusiastic about many of the papers and pleased that so many authors had shared their experience of "wrestling with the beast in the basement," as special editor Fred Campbell so imaginatively labels it. We received more good papers than we could publish in a single issue, and several of them will be published in subsequent issues of TAS.

One disappointment remains. While the topic of undergraduate sociology curricula received a thorough examination, the topic of graduate curricula received hardly any attention. No doubt the priorities of the profession have been affected markedly by trends in undergraduate enrollments. It was surprising, however, to discover the apparent complacency about graduate curricula.

The mixed success of our invitation for papers on curricula leads me to conclude that we should not feel that we have "given at the office" our fair share of attention to curricular matters. Thus we reissue the invitation for colleagues to experiences—successes and with graduate, as well as undergraduate sociology curricula.

As special editor Campbell (who will remain on our board of editors) notes, TAS will be most receptive to those papers that provide information about actual experiences with innovative programs. While it is fun, and occasionally instructive, to conceive of proposals for restructuring curricula, the reviewers and editors clearly favor papers that assess and evaluate innovative programs and practices—not merely describe them.

The next issue of TAS will contain the comments of several persons who spoke at the memorial for Talcott Parsons at the ASA meetings in Boston (August, 1979). These comments offer some poignant observations about a man who, among many other accomplishments, affected the profession and this journal profoundly. Also in the next issue, we will publish a paper by Westie and Kick about sociologists' expectations for immortality.

Finally, for close readers, I should note that in the previous remarks, my switching back and forth between the pronouns I and we does not betoken that an imperial, editorial we is aborning. A real we constitutes the editorial staff of TAS. My three exceptionally capable colleagues—Loretta Williams, George Primov, and John Hall—make our weekly meetings a lively time, marked by sharp discussion and no small amount of disagreement. We represent many points of the compass, professionally and intellectually. When I write to authors, "We have decided . . .", few will appreciate how painstakingly that decision was forged.

For information for contributors, see TAS 15(1, February), inside back cover.

---◆---

THE BEAST IN THE BASEMENT

The call was for papers on curriculum. We have such a beast in my department. It's the mutated offspring of unsuccessful parents and looked old the day it was born. For awhile it lived in the open and fed on large herds of undergraduate students. But as the climate on campus grew cooler, students became more scarce and overspecialized in the wrong direction. The beast could not readapt. Now it seems bound for extinction. A few years ago it stopped hunting and migrated to an unwanted niche in the basement where it hangs on by scavenging the carrion left by the business school. I found it there, hunkered down with its head in a dank corner watching the water from a wet Seattle winter seep through the wall. The failure of the beast is a lesson in curricular evolution.

THE NATURE OF THE BEAST

The beast is made up of the collection of courses offered in our department. It is odd to the eye for it has so many vestigial organs; courses that are relics of some ancestral form without function today. But its main flaw is morphological and is in the arrangement of its courses. Ideally, the beast should be more than the sum of its parts and its structure should produce a separate, larger, educational effect. True, if you look closely there is some sequencing and a hint of hierarchy, but not much more than that. On the whole, its series of single courses don't accumulate too much. This beast should occupy the principal niche in our undergraduate program, but as it is, it contributes little and belongs in the basement.

It is not hard to spot the main reasons that the beast is in such poor shape. First of all, the primary evolutionary mechanism which produces one species of curriculum after another operates in a blind manner. This mechanism is the self-interest of the individual faculty members. Our department, like nature, is a highly utilitarian world and we assume that if each person is allowed to pursue their own ends the greatest good for the greatest number will arise. This economic view is backed by values of academic freedom that invest each person's course with a degree of the sacred. Our position is by no means all bad and for many reasons I would not change the constituent order of things. But it does mean that a curriculum is likely to be little more than just a collection of the courses that people are moved to offer. To build better curricula requires that the faculty engage in a collective effort based on goals that transcend narrow self-interest, and this is difficult to achieve. Just how difficult is strikingly documented in the lead article by Bradshaw and McPherron. In a survey of universities, four-year colleges, and community colleges, they report on the amount of consideration given to curriculum planning, and what steps are taken to solve a number of curricular problems. They conclude that the main stumbling block to curricular improvement is the lack of effort departments are willing to put into this educational enterprise.

The second problem is that we are interested mainly in using the curriculum to clone ourselves. Sociology is now a highly specialized and technical discipline and has as its main goal the advancement of knowledge through scientific study. We have built the bones of the beast out of our scientific interests, and the curriculum is crowded at the upper levels with esoteric courses. More important, as sociological research grew and exercised its dominance over the curriculum, general education was driven into the shadows. We might all agree that competition between our two main functions of producing and distributing knowledge is out of order and that a more symbiotic relationship would be better. But this is not the way things are today. Graduate and undergraduate curricula, which should be based on very different strategies, tend to have a remarkably similar form. The undergraduate curriculum serves well only those few

2

students who choose a sociology major as a first step toward professional sociology. This emphasis might not be so bad, except not very many students want to be clones. At the undergraduate level emphasis is better placed on using what we know to understand the main economic, political, and social forces affecting human society. This strategy is broadly known as liberal, or humanistic, or general education, and is one that can produce many new species of curricula.

The change that will stimulate the development of new curricula is already with us. For a long time we lived in a system characterized by rapid growth. While the baby boom provided more students than could be comfortably accommodated, we were not so concerned with the kind of education offered in our department. The curriculum could represent the narrow interests of the faculty, courses largely could serve our research interests, and enrollments would still remain high. Now, however, the age structure of the population has changed, and we must adapt to a more stable state. Life seems a bit tougher. The competition for students is keen and our enrollments are slipping. Most important, student enrollment now is tied closely to budgets and faculty positions; and suddenly students have become an important source of sustenance. And so in the end it is self-interest that leads us down into the basement to take another look at the beast and to talk about educational goals and the development of new curricula. This volume contains several papers that should be of help.

SOME NEW IDEAS

Imagine youself as omnipotent and then create a new beast that will follow the strategy of general education. This is what Rhodes has done, and I find the result intriguing. To begin, there is a clear attempt to develop the needed symbiotic relationship between research and teaching. Then there is a theme, the development of life skills. Once you know what you are trying to do, the development of core materials becomes possible as does a logic that produces a sequence of learning experiences. I like Rhodes' curriculum

because it would give me a clear scholarly role, is interesting pedagogically, is derived from the material of our discipline, and is directed at broadly educating the undergraduate. Here are shades of Everett Wilson's call for "teaching as a more disciplined search for reliable knowledge." I wonder if it would work. It has never been tried. We are still making statues of the beast out of clay, and while modeling is instructive, it is not definitive.

Now, we come to a new beast called Competence Based Education, which is so strangely different that I don't know what to make of it. This is no clay model, but is already prowling around and doing quite well for itself. The paper by Woolf and Bishop is based on their experience with it at Alverno College. This is a very important paper, for CBE is actively competing for territory now occupied by more traditional curricula, and how it fairs has strong implications for the profession. CBE makes a radical departure from curriculum based on standard academic subjects by substituting a set of competencies which each student has to master. Effort then is transformed from learning subject matter or content to mastering skills or competencies. At Alverno, the sociology department concentrated on teaching the skill of "analytic capacity." I really don't understand what each of these competencies mean, but I believe that those who use the method probably do. I am more concerned with the professional problems which hang like hair all over this beast. There are a lot of tough questions that need to be asked about CBE, and Woolf and Bishop help us to see what many of them are. For my part, I cringe at giving up content for concentration on a competency, for then I might no longer be a sociologist. And I don't want to trade dispensing knowledge for being a facilitator, for then I might no longer be a teacher. No longer a sociologist nor a teacher, I instantly would lose interest in curriculum. In any case, we owe Woolf and Bishop thanks for raising a central question in their paper. Yet they owe the profession a second paper for I would like to know more about how CBE has affected departmental organization, faculty morale and development, and how much the stu-

dents have learned. I am for liberating the beast and building a better curriculum and maybe this is a good way. But not if I have to take its place in that dank niche in the basement.

One of the most infuriating things about the beast is its arrogance. Even in these tougher times it still snaps at every student that comes near it. It was never really clear why students came in the first place. We might make some weak efforts to explain the educational value of sociology. To go another step and make it clear how sociology might have some occupational value was unthinkable to us. But not to our students. They seem to be almost obsessed with the idea of getting a job. (I find students are so concerned with employment that they almost cannot concentrate when I discuss the details of measuring downward mobility.) The failure of the curriculum to respond to students' occupational interests might be related to declining enrollments. In the paper by Green and his colleagues, you can see how it is possible to shape a curriculum responsive to students' employment goals without compromising intellectual standards. I intend to steal almost all of the ideas they present.

Sort through all of sociology, decide what areas are most important, select the most seminal concepts and robust research findings. Put them all together, call them introductory sociology, and the beast draws its first breath. Do it well, for this is big business—more than a half-million people will take introductory sociology next year. What gets taught in this first course is vital to the curriculum. For most students this is the only course that they will take and they should at least glimpse the world through our eyes and see the importance of what we do. For some few it is the first step to a major and they should start with a good grounding in basic concepts. The text book that these students use should represent sociology in its strongest form. Left to ourselves I suppose introductory textbooks would be fairly representative of at least the state of the art. But they are not. The forces that shape our introductory books extend beyond sociology to the publishing industry. Some of the leading text books are not

even written by sociologists but by professional authors. Maybe it's our fault, but I think that much of what we get in introductory sociology books is below the best of the field. In his paper, Perrucci gives much more than just his opinion as he reviews forty textbooks selected over a period of twenty years. He compares them in terms of content, purpose, and how much they reflect the research that is being done in sociology. Read his results and you may become concerned with the books we use to introduce students to the discipline of sociology.

CONCLUSION

The forces that shape the curriculum will continue to operate whether or not we intervene. The educational environment with its stable enrollments and choosy students will push where we cannot lead. Eventually, the beast will make some more-or-less successful adaptations because the welfare, if not the survival, of our department requires it. There are, however, some things which we can do to help ourselves, and they all center around making teaching a more important part of our professional lives.

We might begin by giving educational problems such as curriculum more space in the professional journals and at scholarly meetings. We received many more good papers than we could publish, and some time there should be another opportunity to discuss curricular issues in *The American Sociologist*. To be informative, however, we need to go beyond the description of programs and provide more information on how such programs actually worked. If someone removes introductory sociology from their curriculum, I would like to know how they reorganized their curriculum to fit this change; whether it made teaching upper division courses more difficult, and how it affected enrollment, student satisfaction, and faculty activity. Curricular change should be treated as an experiemnt; and data should be gathered to judge the success of the experiment. There are other things that can be done through our professional organizations. Some place on the program of the regional and national

meetings should be reserved for those who pursue the teaching mission of the profession. We also need some way outside of the journals to exchange ideas, information, and programs. There are two organizations that can help. The Section on Undergraduate Education is now the second largest in the association. It is organized in such a way that all institutions, from large universities to community colleges, are represented. Within the section there is enough breadth of experience, talent, and interest to permit it to address such programmatic problems as curricular development. The Teaching Resource Center, which is now within the ASA, could also serve as a clearing house for new ideas and curricular innovations.

In the end, the problem of the beast lies within our own behavior. We continue to draw a sharp and wholly dysfunctional line between the production and the dissemination of knowledge. In graduate school we breed new generations of sociologists who are ever more technically trained in research, but wholly unskilled and unschooled in matters of teaching. Yet, when they leave us, most will become occupied largely with undergraduate education for their entire careers. And most will not be very good at it, because we put no resources into training them and did not select them for their talent in this area. Evolution is blind, but it is not without direction. The character of the environment favors some adaptations and not others; in the end you get what you select for. If you want to see what we select for, look over your courses, talk to your students, check the enrollment figures. Take a look in you own basement.

Frederick L. Campbell
University of Washington

MANUSCRIPTS FOR THE
ASA ROSE SOCIOLOGY SERIES

Manuscripts (100 to 300 typed pages) are solicited for publication in the ASA *Arnold and Caroline Rose Monograph Series*. The Series welcomes a variety of types of sociological work—qualitative or quantitative empirical studies, and theoretical or methodological treatises. An author should submit three copies of a manuscript for consideration to the Series Editor, Professor Suzanne Keller, Department of Sociology, Princeton University, Princeton, NJ 08540.

ISSUES AND RESOURCES IN UNDERGRADUATE SOCIOLOGY CURRICULUM*

TED K. BRADSHAW
University of California, Berkeley

SHARON M. MCPHERRON
St. Louis Community College at
Florissant Valley

Although most sociologists spend most of their professional time teaching undergraduates, the organization of undergraduate courses has rarely been examined, the needs of sociology teachers have generally been neglected, and the consequences of curriculum design have seldom been discussed. Since 1974, the ASA Section on Undergraduate Education has been working to change this situation by developing programs and resources for teachers and departments wanting help with teaching. During early discussions within the Section on Undergraduate Education, a project, ASA's Project on Teaching Undergraduate Sociology, was proposed to develop resources and stimulate interest in undergraduate teaching, and it was subsequently supported by HEW's Fund for the Improvement of Post-secondary Education. We believe that the sociology profession must begin both to discuss issues of curriculum and teaching and to develop alternative models for the curriculum and courses therein.

This report is an analysis of data collected under the auspices of the Project in a survey of issues and resources in teaching undergraduate sociology. We focus on the curriculum, which we take to be more than simply the list of courses offered by a department. As Daniel Bell said in his discussion of the Columbia University general education program:

> Writing a curriculum, like cooking, can be the prototype of the complete moral act. There is perfect free will. One can put in

whatever one wishes in whatever combination. Yet, in order to know what one has, one has to taste the consequences. And, as in all such acts, there is an ambiguity for evil, in that others who did not share in the original pleasures may have to taste the consequences.

In sum, it is the moral of a cautionary tale (Bell, 1966). This report, too, is a moral and perhaps cautionary tale about the way different educational ingredients are mixed and savored by sociology departments throughout the United States.

Since 1902 a number of assessments of the course offerings of sociology departments have been made which have both helped to define sociology as a discipline and to chart the growth of course offerings (Tolman, 1902; Bernard, 1908; Chapin, 1910; Kennedy and Kennedy, 1942; Podell et al., 1959; Bates and Reid, 1971; Reid and Bates, 1971). Our study is amplified in several ways. The area of inquiry has been expanded from course offerings and the sociology major to general issues of curriculum design and teaching resources. Community colleges, whose importance to undergraduate education cannot be ignored, have been included in the sample. In 1974, they enrolled nearly 2,200,000 degree credit students, and predictions for 1984 are over 2,800,000 (Simon and Frankel, 1976). Finally, the data collected by this survey complements the data from two other surveys conducted by the Project, one on "teacher development activities" (Ewens and Emling, 1977), and the second on the "institutional context" in which undergraduate sociology is taught (Bowker, 1978).

There are precedents for our effort in other disciplines. Some teaching assessments have come in the wake of substantial criticism and some at a time of considerable success. After the shock of

"Sputnik" in the 50s, mathematics and the physical sciences examined their teaching from kindergarten to graduate school, and reformed both curriculum and teaching methods. Psychology launched a study of its undergraduate curriculum in the early 1970s (Kulik, 1975), while granting more baccalaureates in 1975 than any other social science—nearly twenty thousand more than sociology.

Our evaluation of the sociology curriculum stems from feelings both of failure and success. Some argue that the increasing social problems during the last decade—race riots, busing, economic depression, continuing poverty and discrimination, the crises of the cities— necessitate more effective effort from sociology. But sociology has continued to grow during the last several decades, and provides better professional opportunities than many disciplines. In any case, a careful analysis of the sociology curriculum should offer substantial rewards.

CURRICULUM AND CURRICULUM ISSUES IN SOCIOLOGY

A traditional definition of curriculum is a selection of certain items from the body of concepts, theory, and knowledge produced by a discipline, sequentially arranged into courses each with requirements, prerequisites, and stated educational goals. We also consider the rationale or strategies underlying the selection process as part of the curriculum. The curriculum should never be confused with the discipline as it represents only part of the body of knowledge. However, the problems and nature of the discipline are indirectly reflected in issues of curriculum writing. For example, the diversity of subjects embraced by the discipline creates problems for the construction of a manageable curriculum, and the multiplicity of conceptual frameworks complicates the selection of course content.

This report discusses six issues relating to the sociological curriculum, each crucial to the organization of a department, each open to debate and argument, and each generating alternatives and requiring decisions. The first section addresses the way a department determines what con-

stitutes appropriate subject matter to be included in its offerings of sociology courses. This is the problem of boundaries of a curriculum. The second issue is the way concepts are presented within a department's curriculum. The next question concerns the nature of the course which introduces students to the discipline. It is a particularly problematic course because of the great variety of student interests that must be met. The fourth topic deals with the techniques of teaching used by faculty in sociology departments and the tensions between innovation and traditional methods. The fifth issue involves the ways individual courses form a curriculum, and how students progress from the first course to graduation with a sociology major. Finally, departmental and faculty objectives are considered in terms of the overall structure of the departmental teaching effort. We will deal with these topics in order to provide base line data on what is being done in sociology departments and provoke thought about what might be done.

DATA

The data were collected from a questionnaire mailed to a sample of chairpersons of departments of sociology in universities, four-year colleges, and community colleges. Included were all those departments listed in the *Guide to Graduate Departments of Sociology 1976*, published by the American Sociological Association (ASA), a systematic (one in five) random sample of 192 four-year colleges from the ASA list of four-year colleges offering sociology, and a systematic (one in five) random sample of 236 community and junior colleges drawn from the *Community, Junior and Technical College Directory*, published by the American Association of Community and Junior Colleges (AACJC). The sample contained 682 departments located in every state, and included the same institutions surveyed by the other two project groups that studied teacher development resources and the institutional context. In the following report, the responses of community colleges and four-year colleges were weighted by

sample factors to provide accurate summaries applying to all institutions.

The questionnaires were mailed in July, 1976, and during the next eight months, three followup letters and two additional questionnaires were sent to non-respondents. The overall response rate was 65% and thus provides, with only a few exceptions, quite reliable results. Response rates were 77% for university departments, 67% for four-year departments, and 50% for community colleges.

The low response rate from community colleges is typical, but still disappointing. It is hard to contact community colleges because of the variety of organizational structures, the frequency of social science divisions rather than sociology departments, the existence of multiple campuses and branches administered by a centralized office, and the lack of a public list of chairpersons. We mailed to the dean of instruction of every community college in the sample a letter requesting the name of a chairperson or an individual responsible for teaching sociology. Over 75% of the institutions responded with the name, and ultimately 55% returned the questionnaire, a response rate only slightly lower than the university and four-year college sub-sample. In general, schools not responding with a sociologist's name were small and often did not offer sociology courses. Questionnaires addressed simply to the "chairperson" were sent to these

institutions, and this produced only a 19% rate of return.

DEPARTMENTAL AND DISCIPLINARY BOUNDARIES

Alternatives

The first problem is that the boundaries between sociology and other disciplines are unclear. Some argue that sociology is the "queen of the social sciences," encompassing and integrating research in a wide range of areas and concerned with timely issues. Others, however, maintain that sociology is exclusively the study of forms of social organization in modern societies. Many sociologists occupy some position in the ample space between these two stances. Thus, the distinction between sociology and other social sciences is a controversial, if not an impossible, issue to resolve. Nevertheless, teachers must define sociology for students, and departments must make curriculum decisions about courses that are within their domain. There are several ways a department might define the boundaries between itself and other disciplines. The questionnaire presented four alternatives to chairpersons. Responses are shown in Table 1.

The two most frequently selected boundary definitions were "sociology has a distinctive approach and perspective which can be used to study subjects also

TABLE 1. Alternative Boundary Definitions for Sociology Curricula

	Departments (%) ranking statement "Important"	Department (%) ranking one criterion as statement "Most Important"		
	Total Sample	Univ.	4-Yr. College	Community College
Sociology has a distinctive *approach and perspective* which is used to study subjects also studied by other disciplines.	62	49	48	32
Sociology has a *core content* which is clearly sociological but many courses overlap other disciplines.	46	22	19	20
Sociology has a *fixed and distinctive* content which differentiates it from other disciplines.	31	6	12	11
There are no *inherent boundaries* between sociology and other disciplines. What we teach is due to historical developments or institutional politics, not the unique character of sociology.	10	2	3	6
No Response	*	21	15	30

* No response for total sample for above questions = 4%, 3%, 5%, & 6% respectively.

studied by other disciplines," and "sociology has a core content which is clearly sociological but many courses overlap other disciplines." One or the other of these two positions was selected by about two-thirds of the four-year college and university departments, and half of the community colleges. These choices reflect the existence of a "negotiating space" between sociology and other departments which may be reflected in rapidly changing boundaries, interdisciplinary courses, etc. On the other hand, there was only weak support for the extreme statements that "sociology has a fixed and distinctive content which differentiates it from other disciplines," and "There are no inherent boundaries between sociology and other disciplines. What we teach is due to historical developments and institutional politics, not the unique character of sociology."

Cross-Listed Courses

The selection of boundary criteria that encourage permeable and negotiated boundaries with other disciplines leads to courses in which responsibility is shared among a number of departments. The questionnaire listed nine such courses. Not included were courses that previous studies had shown clearly were identified with sociology departments. Departments were asked to respond by indicating courses that were taught by sociology, courses that were taught in other departments, and courses that were taught both by sociology and other departments. If a course was cross-listed, the respondent was to answer in terms of the department that had primary responsibility for the course.

Of the courses listed, sociology has primary responsibility for marriage and the family courses, with 73% of the total weighted sample offering it. Sixty percent of the departments offered ethnic studies, black studies or race relations. Over one third offered organization theory. But fewer of the departments in the sample, 26%, offered courses in sociology of education; 28% in women's studies; and 32% in aging and problems of the elderly. Child development was offered by 15% of the departments.

When the list of courses is examined from the perspective of what courses are most likely to be offered by sociology and a second department, the two most frequently shared courses are social psychology (taught by two departments in 27% of the cases) and statistics (taught by two departments in 23% of the cases). Furthermore, when these two courses are taught by only one department, it is not likely to be sociology. In the case of social psychology, 27% of the institutions offer it in a department other than sociology, compared with 16% in sociology; in the case of statistics, 38% of the institutions offer it in a department other than sociology compared with only 7% exclusively in sociology. Examining the data from universities alone, social psychology is more frequently shared, with 67% reporting that the course is taught in two departments. The same is true for statistics; 59% of the universities offer it in other departments as well as in sociology.

In the case of social psychology, course boundaries between the disciplines of sociology and psychology seem to mirror the position of the speciality in the discipline. The lack of an organized voice within the ASA has plagued social psychology for some time. The concern of the discipline about research in sociological social psychology occupied twenty pages of a recent issue of The American Sociologist. Alan Liska writes therein: "To summarize, the dissipation of social psychology directly observable in the citation patterns of sociologists doing social psychology, may also be reflected in the recent content analysis of leading sociological journals, the content and paucity of social psychology textbooks authored by sociologists, and the apparent lack of concern by psychologists for the work of sociologists" (Liska, 1974:4).

Interdisciplinary Involvement

Sociology's amorphous boundaries create a structural tendency toward interdisciplinary programs. Indeed, many sociology departments are actively involved in them. While only 16% of the departments reported no such involvement, 39% reported it was either "com-

pletely" or "generally" descriptive to say that they were involved.

The fact that 46% of the four-year colleges reported active interdisciplinary activity reflects a traditional commitment to liberal arts and an emphasis on integration of knowledge. Only 38% of the universities reported engaging in interdisciplinary programs, and—somewhat surprisingly—only 32% of the community colleges so reported, despite such structures as social science divisions and behavioral science departments which restrict opportunities for narrowly disciplinary activities. It may be that in the case of community colleges, the need to maintain identity in a structurally confusing situation is stronger than the desire to innovate with boundaries (Stein, 1977).

Sociology Comes of Age

Although it is a new discipline, sociology seems to have come of age in academic institutions. Only 14% of the departments described themselves as marginal, threatened, and/or misunderstood. Seventy-six percent of the departments felt that when new programs were developed, sociology's participation would not be blocked by other departments, and 53% reported that they thought they could teach any course they wanted and were competent to offer. Thus, new courses are blocked less by rival departments than by institutional restraints of budget, facilities, and enrollments. Disciplinary soul-searching about boundaries is apparently not communicated to beginning students. No single department chairperson was willing to agree entirely with the statement, "after completing their first sociology course, students expressed concern that they can't distinguish sociology from the other disciplines." In fact, 68% of the respondents indicated that the statement was not at all descriptive of their department. It is possible that department chairpersons are out of touch with student perceptions, but we think not. Criticism of the curriculum does reach the chairperson, particularly in the four-year and community colleges in which the chairperson is the first court of

appeal when students are unhappy. The percentage of agreement is even higher in these departments than in universities where the chairpersons are further removed from undergraduate student complaints.

With declining student enrollments and concern about them, departmental competition at their boundaries for courses and students is to be expected. Survey results indicate that competition is there, and that it produces conflict on occasion. Twenty-five percent of the departments in the weighted sample reported that during the last five years they had been involved in a dispute or misunderstanding with other departments over course offerings. Sometimes these disputes were minor misunderstandings, but in other cases, they could not be resolved and neither department offered the course.

As one might expect, the majority of the reported disputes involved questions of "turf" with other human science departments. Social psychology was the most frequently reported area of controversy; estimates indicate this course is a matter for dispute in about 10% of the cases. The next largest problem area was Marriage and Family, mostly involving home economics but occasionally a psychology department. Political science departments were involved in disputes about deviance, criminal justice, and political sociology. Other institutions reported courses on human sexuality or intimacy as areas of disagreement involving biology, psychology, and the school's curriculum committee. Less frequent disputes concerned urbanization, aging, sociology of education, sociology of sports and recreation, technology and society, minorities, and third world studies.

Universities, with their larger range of courses and programs, were the most likely institution to have disputes: 37% of university departments reported occurrences compared with 27% of colleges, and 18% of community colleges. The fact that there are fewer disputes in the four-year colleges could possibly be explained by their greater effort to be interdisciplinary, and the paucity of disputes in a

community college may be the result of less rigid departmental organization.

In summary, sociology departments set up boundaries that create a significant hiatus between themselves and other departments. The boundary definitions are consistent with the data reporting a significant number of shared courses, participation in interdisciplinary programs, disputes over courses, and competition for students. But despite disputes and competition, sociology seems confident of its position among academic departments, and whatever the ambiguity felt by the discipline, it is not shared with students.

THE ROLE OF CONCEPTS IN TEACHING AND CURRICULUM

The second issue in sociological curriculum involves the need to establish better conceptual foundations. The overwhelming response of the departments in the sample to questions about the place in the curriculum of concepts indicates their enormous importance to teaching. Of the departments responding, 35% found considerable, and 42% moderate, agreement with the statement, "If a student learns a concept well then we have done the most important part of teaching sociology." The responses followed a similar pattern in reaction to a parallel statement, "One must be conscious about the importance of teaching concepts in sociology." Considerable agreement was expressed by 41%, and moderate agreement by 47% of the respondents. Four-year colleges were somewhat more likely to agree, 44%, than universities, 31%, or community colleges, 36%.

In stark contrast to their commitment to concepts is the unwillingness of departments to be specific about this essential component of teaching. When asked if the department had a list of concepts that should be taught in the first course, 87% replied no; when asked about other courses, 80% replied no; and when asked about concepts expected to be mastered before graduation, 93% replied no. There are many reasons for the absence of a list: the youth of sociology, the variety of paradigms, and fear of infringing on academic freedom and of stultification by prescription. But, the survey data indicate that in addition to these factors there is a lack of agreement among faculty about the way concepts are defined and sequentially arranged. The departments were asked to respond to the statement, "Faculty here are in agreement about how concepts are elaborated and built one upon another as students progress in the department." A negligible number, 7%, indicated that the statement was completely descriptive, while only 40% could respond that the statement was even generally descriptive.

The next concern is the source of materials for teaching sociological concepts. When asked about texts as a source of information, only 4% of the department chairpersons responded that it would describe instruction in their department to say that "we rely on texts for the presentation of most concepts." Another 36% of the sample replied that it would be generally descriptive, while 55% of the sample answered that it would describe instruction in their department only "partially" or "not at all." The lack of trust in texts for the presentation of concepts, particularly given the importance of concepts to teaching, is disturbing, considering how texts dominate all instructional media in introductory courses and in other courses · as well.

If texts are not used, perhaps faculty help each other develop better methods of teaching concepts. Nevertheless, survey results show that during faculty meetings, . the importance and methods of teaching concepts were discussed infrequently; only 23% of the respondents found a statement to that effect completely or generally descriptive. If texts are untrustworthy and faculty discussions of concepts infrequent, faculty members must rely on their own resources. In such a situation, the teaching is only as good as the teacher. Thus, while concepts are seen as the key to teaching well or the rod by which faculty and departments measure successful teaching, disagreement about sequence, distrust of texts, lack of discussion, and a general unwillingness to rationalize the teaching of concepts still exist.

THE FIRST COURSE

Introduction to Sociology

Virtually every sociology department in the United States offers a general introductory course as the first course in its curriculum. This first sociology course constitutes a large part of the teaching efforts of all departments. The sampled departments reported a mean of 10.8 sections per department offered during the school year 1975–1976. On the average, a department enrolled 482 students in the first course, and the average faculty member taught 3.1 sections of the course during the school year.

Goals and Objectives

Given the lack of agreement about the direction and goals of sociology as a discipline, it is not surprising that there is a difference of opinion about the nature of the first course. The questionnaire asked departments to respond to five statements of goals for the first course. The responses are in Table 2. Respondents were asked to indicate how well each statement describes their department's goals for the first course, and also, to select the one statement that most accurately described their department's goals. The goal rated as "most important" for courses across all institutions was, "we emphasize teaching

fundamental concepts which will be needed in subsequent sociology courses." This statement was also selected as most descriptive of the first course in nearly three-quarters of all institutions. This concern with providing a foundation for subsequent study is consistent not only with a belief that there is some structure for the learning of sociology, but reemphasizes the importance of concepts for the curriculum. Note from Table 2 that community colleges rank this goal lower than do universities and four-year colleges. This represents increased emphasis on other alternatives in community colleges as well as the general lack of subsequent courses that students might take.

Another goal for the first course is to provide students with a sampler of the multiple perspectives, concepts, and substantive areas included in the field of sociology. Responses to our survey indicate that many of the departments think that introducing students to the subjects that sociologists study, 62%, and to a variety of sociological perspectives, 61%, describes well their goals for the first course.

In contrast to the enthusiasm for presenting students with a variety of subjects, most departments were less interested in teaching first courses that either emphasized "helping students to understand everyday life," or "learning

TABLE 2. Departmental Goals for the First Sociology Course: Percent Indicating Statement Describes the Goals Well And Percent Ranking Each Goal Most Accurate by Institution Type

Goals	Departments (%) ranking statement "Important"	Departments (%) ranking one goal as "Most Important"		
	Total Sample	Univ.	4-Yr. College	Community College
We emphasize teaching fundamental concepts which will be needed in subsequent sociology courses.	74	45	39	28
We give students an introduction to the subjects sociologists study.	62	15	12	14
We try to introduce students to a variety of alternative sociological perspectives.	61	16	18	11
We try to help students understand everyday life.	50	5	11	24
We want the students to learn the most important scientifically established facts from sociological research.	26	2	2	1
No Response	*	12	15	19

* No response for above questions = 1%, 2%, 1%, 2%, 2% respectively.

the most important scientifically established facts from sociological research." The attempt to help students understand everyday life describes well the first course in only about one-half of all departments. Very few university and four-year college departments indicated this was their most important goal. Nor do departments emphasize teaching sociological facts; only about a quarter of all departments feel that this goal describes their program well. One would expect community colleges to rank research findings low, both because they rate goals of personal adjustment higher and because their faculty is less likely to do research. It is more surprising that only 2% of departments in universities and four-year colleges rank transmission of research findings as most important. Given the commitment of the discipline to research, the low ranking given to teaching research findings documents an ironic discontinuity from priority in the discipline to priority in curriculum.

The Audience

The audiences needing an introduction to sociology challenge any attempt to standardize first courses. In a number of schools, introductory sociology is required of students in other programs, and they constitute a large portion of the department's enrollment. Nursing and social work programs require the introductory course offered by 51% and 54% respectively of the departments responding to the questionnaire. In 18% of the community college sample and 15% of the total sample, students from these fields comprise *over 50% of the students who take the course*. Thus, sociology provides a substantial service to other departments, and is not simply in the business of providing an introduction for sociology majors.

Very few institutions offer special sections of the first course for students from other departments or for students who are not likely to take another sociology course, but there is some flexibility. Though 90% of the departments that completed the questionnaire reported students usually took introduction to sociology or

principles of sociology, 45% indicated that students *could* select something other than the usual first course. Some of the options included Social Problems, the Family, or occasionally Criminology, as a first course, while others removed the prerequisities from a large number of courses usually taken in the first two years. Thus, while introduction to sociology is firmly entrenched as the *usual* first course, other options provide variety in nearly half of the departments.

Diversity typically has not been encouraged in the usual first course. Less than 1% of the departments report that students may generally choose texts, assignments, sections, and/or modules on the basis of their interest. In contrast, nearly 37% of the departments generally specify a common text or reader for all course sections at their institutions. In addition, 17% report that all sections generally share a common first course examination. The standardization of the first course is an important countertrend to the diversity of goals across departments. It undoubtedly reflects the pressures of faculty to lighten their workload, and departments, their administrative duties. These pressures are most acute in the community college, where the standardization is more evident. Of the community college departments, 54% generally specified a common text and over 25% share common examinations.

Problems of the First Course

Problems surrounding the first course frequently were mentioned to us during the preparation of the questionnaire. Almost half the respondents think that students do not receive from the first course what the departments think they should, but only 2% thought the lack was serious. A more substantial problem in many departments is that students do not have adequate reading and writing skills for the first course. About 25% of the departments reported that this was a serious problem, 57% said that it was somewhat of a problem; only 18% of the departments said that it was not a problem. The difficulty was more acute in community colleges, with 29% characterizing it as seri-

ous. Four-year colleges rated it serious in 21% of the responses and university departments in 16% of the responses.

Nevertheless, survey results suggest that most concerns about the first course may have been overstated. The majority of the sample rejected statements that there are too few liberal arts students in the first course; that it is a bottleneck for many students wanting to take subsequent sociology courses; that it prepares students inadequately for subsequent courses; that most faculty, squeezed for time, give it less attention than it deserves; or that the faculty do not want to teach the course. The latter problem was held to be generally or completely descriptive in only 16% of the departments, and over one-third said it was not descriptive at all. Department chairpersons did not portray a first course beset with problems. While a different impression might have emerged from the faculty them-

selves, the data show the first course to be healthy in most institutions.

INNOVATIVE TEACHING TECHNIQUES

In sociology the teaching atmosphere is alive with new experiments to deliver better learning experiences. Departments were asked to specify how much they used a number of different teaching techniques and to indicate their awareness and use of modules, as a detailed case study to evidence awareness of innovations.

As a baseline, the data presented in Table 3 show that the majority of departments continue to place considerable reliance on what may be called traditional teaching materials such as texts, other books, term papers, objective exams, and written and oral examinations. In the first course, over 90% of departments make use of texts and 60% of objective exams,

TABLE 3. Percentage of Departments Using Selected Teaching Techniques*

	First Course			Other Courses		
	Extensive	Occasional	Not At All	Extensive	Occasional	Not At All
Traditional Teaching Material						
Textbooks	90%	7%	2%	75%	17%	1%
Other books	35%	60%	5%	55%	37%	17%
Term papers	15%	54%	27%	44%	43%	5%
Objective exams	60%	32%	5%	40%	47%	5%
Essay or Oral exams	25%	61%	11%	46%	43%	2%
Established Innovations						
Field experience	6%	66%	25%	18%	69%	6%
Films	19%	76%	3%	18%	71%	2%
Independent Study and Research	6%	63%	27%	19%	67%	4%
Simulation and Gaming	3%	53%	44%	2%	61%	26%
Individualized Teaching Techniques						
College Level Exam. Program (CLEP)	1%	37%	53%	2%	24%	62%
Experiential Learning Exercises (role playing, sociodrama)	7%	63%	24%	8%	68%	16%
Modules	3%	21%	67%	3%	28%	56%
Programmed Learning		21%	72%	1%	25%	62%
Self-paced Study	2%	22%	69%	2%	36%	50%
Equipment Heavy Innovation						
Integrated Use of Multimedia	5%	44%	44%	5%	47%	37%
Televised Presentations	1%	33%	69%	2%	37%	49%
Audio Tapes	3%	59%	33%	5%	65%	19%
Computer-assisted Instruction		18%	74%	2%	31%	56%

* No response varies from less than 1% to 9% for individual items.
* Weighted Responses.

the most concentrated use of any of the techniques reported. In subsequent courses there is significantly greater use of other books, term papers, and essay and oral exams. There are some important differences in the use of the teaching techniques among ·types of institutions. Community colleges, compared to both four-year colleges and universities, rely more on textbooks, assign other books less often, demand fewer term papers, and give fewer essay or oral exams. One exception to the general trend is that universities are the largest user of objective exams in the first course.

The second group of teaching techniques is what we call "established innovations," including field experience, films, audio tapes, independent study and research, and simulations and games. Virtually every department makes use of films; they are used in 95% of all first courses and in 89% of others. Teachers of sociology are surprisingly heavy consumers of educational films, considering that journals offer. almost no space for review of films, professional meetings do not preview or discuss them, and neither literature nor workshops encourage their creative use. Surely the extensive use of film documented in the survey argues for increasing professional attention to better utilization of the medium. Both field experience and independent study are put to use in the first course by over 60% of departments, but the frequency has not generated professional guidelines for their design. In contrast gaming and simulation—used by over 50% of the sample in the first course, and over 60% in other courses—has its own journal, *Simulation & Games*.

Compared with traditional teaching materials such as texts, the use of established innovations is much less "extensive" and more often "occasional." It would be surprising if any department used them a lot in any but a most extraordinary experimental course.

A third category of innovation, "individualized teaching techniques," reaches modest but significant levels of use in more advanced courses and minimal use in the first course. They include College Level Examination Program (CLEP)

exams, experiential learning exercises, modules, programmed learning, and self-paced study. Similar results were obtained for the fourth category, which we called "equipment heavy innovations," including integrated use of multimedia, television presentations, audio tapes, and computer-assisted instruction. A surprising percentage of departments occasionally use these two categories of innovations, which require special·teacher skills and equipment. At least a third of all institutions have teachers using most of these new techniques.

From these data, two conclusions can be drawn: (1) awareness of innovative techniques, at least to the extent of occasional use, is widespread; and (2) initial enthusiasm for technological innovations and heavy financial investment in equipment such as computer terminals and television have not led to the domination of pedagogy by machines. For every attempt to use mass processing techniques, there are attempts to use individualized learning processes.

Many people have hoped that modules would provide increased flexibility to respond to the diversity of student interests. Using modules as an indicator of the spread of innovation, departments were asked a series of questions about their general interest in modules, how much they use them now, and plans for future use. The data show that 20% of all departments report that they were using or will use modular units "where the learner actively participates in ways other than simply reading, listening, or discussing things in class." Another 31% indicated that they will experiment with these modules. As shown in Table 4, more structured module packages received less interest, but none was rejected outright. There is a great interest in modules, as probably is true for most innovations, yet most departments have taken a slow, cautious approach to adopting them. The data suggest that the media which distribute information about innovations are functioning well in sociology. Still, dissemination of information appears less important than encouragement for the use of innovations and subsequent evaluation of their efficacy.

TABLE 4. Extent To Which Departments Are Interested in Modular Techniques For Teaching Sociology At All Levels

	Will Use or Are Using	Will Experiment	Will Get Information	Not Interested
Units where the learner actively participates in ways other than simply reading, listening or discussing things in class.	20%	31%	23%	15%
Units where objectives are spelled out and tied to evaluation to indicate achievement of objective.	14%	24%	29%	23%
Self-contained packages pertaining to single concepts or units of subject matter which may be separately mastered.	9%	23%	30%	28%
Units which may be used at different rates by individual students.	8%	24%	33%	26%
Self-instructional packages using specially prepared materials.	9%	22%	33%	26%

No response varies from 10% to 11%

ORGANIZATION OF THE CURRICULUM

To this point, our analysis has concentrated on the content, methods, and problems of individual courses rather than on the way they are organized into a curriculum. In this and the following sections, attention is turned to the rationales and strategies by which courses are arranged into sequences and to the goals or outcomes of the curriculum. The consequences of each teaching decision are multiplied in the curriculum, and so the difficulties of teaching sociology courses are seen in sharper focus in curriculum decisions.

Curriculum decisions have to be made for students taking a single course as well as for those majoring in sociology. Nonetheless, the major is the single most important unit for the organization of curriculum in sociology departments in universities and four-year colleges. In university departments a sociology major is required to take an average of 10 courses, with an average of 4.7 of them specified, as opposed to elective. In four-year colleges, an average of 10.8 courses are required, with 4.7 specified.

Sociology concentrations or majors leading to an Associate of Arts degree are available in only 18% of the community colleges. The limited number of schools with majors is to be expected. The surprise is that general community colleges have relatively large and varied programs in sociology, averaging a total of 6.9 different courses in each college. These courses are sometimes organized into specializations outside the major, as in social or behavioral sciences, and in social service programs.

The curriculum in most institutions is not a fixed pattern of progression through a series of undergraduate courses. A moderate amount of change takes place in the sociology curriculum across the nation, with 15% of the departments reporting recent major changes, and 30% reporting minor changes or revisions. A review of the changes indicates that most of the minor changes are attempts to prepare for the contingencies of life. Courses were frequently added in health, aging, death and dying, and sex roles. Courses typically deleted include social work, public opinion, and demography. Furthermore, many departments made changes in the theory-methodology area, adding or dropping prerequisites for advanced courses. These latter areas are frequently mentioned as sources of problems in the curriculum at many institutions.

In general, most institutions reported that it was easy to add courses to the curriculum. On the other hand, over 40% of community colleges reported that it was hard. Some of the difficulty comes from the overall limitation of resources. In addition, it is typical for community college departments to have to demonstrate that local four-year colleges will accept their curriculum changes. Community colleges report that the requirements of

other institutions constitute a major curriculum constraint, felt by 50% of them. Constraints in curriculum change undoubtedly are the source of morale problems in community college departments that cannot respond to the changing needs of those they serve, and who are called on to enroll increasing numbers of students for their first two years of training. In contrast, only 15% of the four-year colleges and 14% of the universities report these problems. The constraint mentioned by one university respondent was that transfer students coming to their institutions were ill-prepared and poorly counseled with regard to sociology offerings.

The changes that are indicated in the survey do not reflect either an inclination toward or opportunity for a fundamental reorganization of the curriculum. There are many more experiments with technics. But it might well be that teaching could be improved more economically by careful review of curriculum than by investment in new teaching technologies.

Since the curriculum serves a variety of purposes both for the student and the institution, change therein involves choices among a number of views of the way sociological knowledge should be acquired. Several alternatives were presented to survey respondents. The most frequently selected alternative (52%) is the statement that "Sociological knowledge is relative to the student's career, academic, and personal goals. Thus, courses should be structured to meet these goals." This "pot-pourri" approach reflects the character of sociology as a discipline in which there is no single line of cumulative knowledge as there is, for example, in mathematics. The second

most frequently endorsed choice is, "Sociological knowledge can be obtained in any order, just so long as a certain amount is obtained before graduation." This statement was selected by 22% of the departments as the most important consideration with regard to the curriculum, and represents a "full glass" approach to sociological training. The third choice selected as most important by 17% of the departments, suggests that "Sociological knowledge can only evolve from a single entry point and move through a specific set of courses, each buliding upon the one before it." It is interesting that this "knowledge chain" approach is so weakly endorsed, considering that sociologists often express the view that certain core concepts must be mastered before students can take advanced courses.

There may not be any standard form of organization, but most departments still think that the curriculum is important for achieving educational goals. Table 5 shows the response of different types of institutions to questions about the components of a curriculum. The most frequently indicated goal for the undergraduate curriculum is the development of critical thinking by students. This is followed by an emphasis on sociology's contribution to an overall general education, and the development of cultural awareness among students. These general goals are more important than a mastery of sociological knowledge, and the training of professional social scientists. Finally, sociology departments in the mid-1970s do not emphasize the goals of growth, creativity, and diverse student interests. There remains a predisposition to respond to the goals of a general educa-

TABLE 5. Components of an Undergraduate Curriculum: Percent Indicating Absolutely Essential to Their Department

	Univ.	4 Yr. College	Community College
Development of critical thinking	47%	53%	35%
Sociology's contribution to an overall general education	45%	47%	30%
Development of cultural awareness	27%	36%	38%
Mastery of a body of sociological knowledge	22%	37%	18%
Training of professional social scientists	10%	16%	13%
Facilitation of personal growth	7%	19%	24%
Development of student creativity	6%	14%	11%
Satisfaction of diverse student interests	6%	8%	17%

tion rather than to what used to be called relevance or individual fulfillment. This does not mean that departments feel the latter to be "definitely not desired"— virtually no department responded this way, and only about one in ten departments responded that these goals were "not important."

GOALS AND OBJECTIVES

There is increasing pressure in many institutions for departments to specify their goals and objectives in written form, and to measure the adequacy of departmental programs relative to these goals and objectives. This development is consistent with the general trend in higher education toward accountability, but at the same time may reflect a consensus among sociologists that a rational set of goals and objectives improves teaching and provides guidelines by which administrative decisions can be evaluated.

Two objections to setting goals often are raised: first, a department might be held accountable for its own statement of goals, and second, an institution might not permit development of the individual departmental goals. Respondents were asked to agree or disagree with a statement that read, "The administration will likely impose unacceptable demands on the department if we develop a set of formal objectives." Only 1% of the department chairpersons strongly agreed, 13% expressed moderate agreement, while 77% expressed disagreement. Chairpersons were asked to comment on how well the following statement described their department, "This college has an educational philosophy which leaves little room for a department to develop its own objectives." Of the departments responding, only 3% indicated the statement was completely descriptive, 8% generally descriptive, 34% partially descriptive, and 52% rejected the statement as not at all descriptive. Thus, neither of the two common objections to setting specific goals and objectives were actually perceived as problems by departmental chairpersons.

The educational literature argues that the determination of objectives improves teaching, and the majority of sociology departments agree. Respondents were asked to express agreement or disagreement with the statement, "Specific educational goals will improve the way individual faculty teach sociology." Twenty-one percent of the sample expressed strong agreement and 53% moderate agreement; 18% disagreed moderately and only 2% strongly. But when asked for an opinion based on their own experience whether departments should write specific objectives, only 37% said yes, while 32% responded no, and 31% indicated no experience from which to speak. When asked why they felt the way they did, those who favored setting goals cited the opportunity to bring unity to the department by increased accountability, to improve performance, to avoid distractions, and to stimulate discussion and thought. Those who opposed the use of formal goals mentioned that setting goals would lead to considerable dissension in the department, waste a lot of effort, create reams of useless paperwork, and institute processes that would hinder rather than promote good teaching.

From the survey data we can make three further observations on the educational process. First, sociologists reject the behavioral approach to educational objectives, with 78% of departments agreeing with the statement, "Education cannot be reduced to a series of units to be separately mastered. It is instead a complex whole." Furthermore, 75% of the respondents supported the position that objectives or goals of a department must be set by faculty consensus. Thirdly, departments were asked how well the following statement described their department, "We collect or examine data about the interest and needs of our students when we develop curriculum." Only 11% of the sample indicated it was "completely descriptive," and 33% indicated it was "generally descriptive," leaving over half who found the statement minimally or not at all descriptive. If curriculum were a problem of social policy, sociologists would be forced to give themselves low marks on the utilization of data in planning.

Sociology departments expressed a great deal of interest in developing goals

and objectives as a way to reassess their work, but most hesitate to establish specific objectives beyond a teacher's own efforts in a single class. Furthermore, departments do not tend to hold individual faculty accountable for setting their own teaching goals. Just slightly over a third of the departments report that the department chairperson or dean meets with each faculty member to discuss teaching objectives. In short, sociology departments have been cautious about setting specific goals and objectives to facilitate their curricular developments.

CONCLUSION

Where are we?

A general picture of the teaching of sociology begins to emerge. Sociologists define the discipline and curriculum in statements open to interpretation and debate; have no preconceived notion of an entry point to the body of knowledge that is defined as sociology; do not think that concepts need be arranged in a sequence that requires the mastery of one before the other. For these reasons it is difficult for sociology departments to lay claim to certain areas of "turf," for it cannot be argued that a disputed course clearly belongs in a sequence, or that the curriculum would be incomplete or lack internal consistency without it.

In the time since the data were gathered, they have been supplemented with observations gleaned from visits to numerous departments, discussions with deans and other administrators, and participation with sociologists in workshops on curriculum. It is very clear that deans and other administrators view sociology curricula as unplanned, and they are demanding that changes be made. Nevertheless, deans and departmental administrators wish for very different results from reorganization or curriculum review. Deans want order; departments hope to add courses and options. Confusion is compounded.

Declining enrollments, fewer majors, the increasing popularity of human services, criminal justice, and social work add evidence that sociology needs to reconsider curriculum. If there is to be a change, departments of sociology should use the same skills that sociologists urge on the groups for whom they act as consultants in the design and evaluation of social policy and programs. Data gathering is always the first step, but the response of sociologists to planning curricula has always been that no data exist, or that the issues cannot be operationally defined. While no single survey can hope to provide definitive data, and while this one has serious omissions, we have begun to collect information that can be used to plan curricula. And this survey does not stand alone, but is one of three fielded by the ASA Projects on Teaching Undergraduate Sociology. In addition to these surveys, there is an ample supply of government data projecting trends and enrollments in higher education. There is significant literature on higher education, much of it produced by sociologists, and the discipline itself has a history of curriculum planning beginning with Tolman, and continuing to Bates and Reid. The discipline, in this special issue of *The American Sociologist* and in past issues of *Teaching Sociology,* has begun to make available numerous models for course and curriculum planning. We now have more data about teaching and curriculum than we have ever had. There is more than ever will be analyzed, or utilized, or even read. There is no reason that planning, informed by data, cannot begin.

That institutions block changes in curricula is the second lament of sociology departments. In the questionnaire, departments were asked about this problem. There were statements about the marginality of sociology in an institution, degree of difficulty involved in proposing new courses, narrowness of the college's educational philosophy, questions about the tactics of other departments, and the actual number of sociology departments that had disputes with another department. None of the statements received significant support, and only a quarter of the sample reported disputes with other departments. Departments thus lent no support to the theory that change is not possible because of institutional constraints.

If there is no lack of data and materials

for planning, and institutions do not discourage change, then the climate of the department should be examined as the reason for inaction. A survey of the institutional context of teaching (Bowker, 1978) reported that chairpersons thought it appropriate that about 5% to 7% of their time be spent on curriculum issues. In this survey, only about a quarter of the surveyed departments ranked curriculum as their most pressing problem, and less than a quarter reported that it was the object of departmental effort. Joining these data with those from the institutional context survey presents a still more interesting state of affairs. Of the department chairpersons sampled in that survey, 27% supported the statement that the ASA could be most useful to them by establishing guidelines for undergraduate sociology curricula, and an equal number ranked it second in a list of five items. Not only is very little departmental time spent on issues of curriculum, but support for ASA guidelines is quite strong. There appears to be a willingness to leave the task to others.

Throughout the survey, departments consistently rejected all statements suggesting that something intrinsic in sociology can give direction to sociology curricula. Likewise, they rejected the process of setting goals and objectives, which represents a planning strategy to impose order on the curriculum. In addition, the data on teaching innovations indicated that traditional teaching styles clearly dominate in sociology. All of these are indicators that perhaps it is the internal value systems of departments that block change in curricula.

In the past, when troubles internal to the discipline have been diagnosed, sociologists' reaction has been to answer that this is the price to be paid for intellectual variety. This reaction to the need to change the curriculum can be different. We have defined curriculum as the result of a rational process, related to, but not the same as the discipline. Curriculum is a separate, distinct, entity. It can be planned, improved, have direction, have goals, objectives, and structure, even though the discipline is without clear direction. The goals and objectives of the

curriculum and its structure must be derived from the core of the discipline, but they have a separate, objective existence.

Sociologists have at their command professional skills that equip them to begin major restructuring of curricula. They have a body of knowledge about formal organization in higher education, and the complex relationships between groups and socialization. They have data gathering skills. If sociologists can redefine curricular problems in terms of social systems, and if they can rationally plan and evaluate their curricula, they will have a marketable skill in higher education as well as the coherent curriculum they need.

REFERENCES

American Sociological Association
   1975   Guide to Graduate Departments of Sociology, 1975. Washington, D.C.
Bates, A. P. and S. T. Reid
   1971   "The sociology major in accredited colleges and universities." American Sociologist 6:243–249.
Bernard, Luther
   1909   "The teaching of sociology in institutions of learning in the United States." American Journal of Sociology 15:797–838.
Bowker, Lee H.
   1978   Data Book on the Institutional Context of Teaching of Undergraduate Sociology. Washington, D.C.: Teaching Resource Center, American Sociological Association.
Bradshaw, Ted and Sharon McPherron
   1978   Data Report: Issues and Resources in the Undergraduate Curriculum. Washington, D.C.: Teaching Resource Center, American Sociological Association.
Chapin, F. S.
   1911   "Report of questions of committee on teaching." American Journal of Sociology 16:774–793.
Drake, Sandra L. (editor)
   1975   1975 Community, Junior and Technical College Directory. Washington, D.C.: American Association of Community and Junior Colleges.
Ewens, Bill and Diane Emling
   1975   "Teacher development in sociology: A survey of current practices and opinions." A report prepared by the Task Group on Teacher Development, ASA's Project on Teaching Undergraduate Sociology. Washington, D.C.
Kennedy, R. and R. J. Kennedy
   1942   "Sociology in American colleges." American Sociological Review 6:667–675.
Kulik, James A.
   1973   "Undergraduate Education in Psychology." Washington, D.C.: American Psychological Association.

Liska, Allan E.
1977   "The dissipation of sociological social psy-
       chology." The American Sociologist
       12:2–8.
Podell, L., M. Vogelfanger, and R. Rogers
1959   "Sociology in American colleges—Fifteen
       years later." American Sociological Re-
       view 24:87–95.
Reid, S. T. and A. P. Bates
1971   "Undergraduate sociology programs in ac-
       credited colleges and universities." Ameri-
       can Sociologist 6:162–175.

Simon, Kenneth and Martin M. Frankel
1976   "Projections of Educational Statistics to
       1984–1985." Washington, D.C.: U.S. Gov-
       ernment Printing Office.
Stein,. Nancy Wendlandt
1977   "Just what is sociology: The view from the
       community college." Teaching Sociology
       5:7–14.
Tolman, F.
1902   "The study of sociology in institutions of
       learning in the United States." American
       Journal of Sociology 7:164–213.

UNDERGRADUATE SOCIOLOGY CURRICULUM: A PROPOSAL*

LAWRENCE J. RHOADES

American Sociological Association

*This article attempts to demonstrate the central importance of the undergraduate curriculum
for the discipline and the profession of sociology. Attention is focused on the current state of the
curriculum, which presently is viewed as dysfunctional for the discipline and the profession. The
article ranges far beyond what normally are considered curriculum matters, and points to the
interdependency that exists between the curriculum, the discipline, and the profession with its
constituent units–individuals and collectivities. The article also proposes a fundamentally new
undergraduate curriculum that calls for scholarship from teachers that would assist students to
develop needed life skills, and that would incorporate teachers into the development of the
discipline as an integrated body of knowledge.*

Although the undergraduate curriculum
is of central importance to the discipline
and the profession of sociology, its cur-
rent state is largely dysfunctional for both.
It is dysfunctional for the discipline be-
cause it excludes most sociologists from
participating in the production of knowl-
edge by relegating them to the transmis-
sion of knowledge at an elementary level
in narrow specialties, thereby retarding
the growth of general theory in sociology,
and creating a debilitating dichotomy be-
tween teaching and research. It is dys-
functional for the profession because it
prevents us from ably serving our clients,
thereby failing to develop broad public
support for the profession.

Some of the inadequacies of the under-
graduate curriculum in sociology were

spelled out in Reid and Bates (1971) and
Bates and Reid (1971). The Curriculum
Task Group of the ASA Projects on
Teaching Undergraduate Sociology (1977)
also addressed the curriculum question in
a special issue of another journal. Davis et
al., (1977) have annotated 17 additional
articles and publications that deal with
various aspects of the undergraduate cur-
riculum in sociology. Nevertheless, cur-
riculum change to this point has been
piecemeal and minor.

A number of pressures, from without
and within the discipline and the profes-
sion, indicate that a fundamental change
in the undergraduate curriculum may be
prudent. These pressures are: (1) the reem-
phasis on general education at the col-
legiate level; (2) the increasing vocational
orientation of students; (3) the identity
crisis in sociology (Stryker, 1979), and (4)
the uncertain connection between sociol-
ogy and social policy (Gibbs, 1979; Na-
tional Research Council, 1978). This arti-
cle addresses these pressures by propos-
ing a fundamentally new undergraduate

curriculum that calls upon teachers of sociology to exercise scholarship on behalf of their discipline and their students. I hope that the proposal will stimulate full discussion of this important matter.

REDEFINING THE CURRICULUM

The central importance of the undergraduate curriculum for the discipline and the profession of sociology is not readily recognizable if the concept of curriculum is narrowly defined as the rationale for course offerings or as what students must learn. These definitions hide the centrality of the undergraduate curriculum more than they reveal it. *The undergraduate curriculum is central to the discipline and the profession of sociology because it is the major structure for ordering the work lives of most sociologists.*[1] Consequently, the curriculum must be examined to determine its relationship to the discipline, to the individuals and collectivities that constitute the profession, as well as to students in search of life skills.

Since the above terms are used repeatedly in this article as well as in the accompanying conceptual maps, it is advisable to define them as used herein:

Discipline: The discipline of sociology is a body of knowledge—theories, propositions, principles, concepts, perspectives, and facts. Its growth and refinement require empirical research and scholarship. *Scholarship* is defined as synthesis, interpretation, and evaluation of the body of knowledge.

Profession: The profession of sociology is the social organization of individuals and collectivities that produce, transmit and apply knowledge.

Individuals: Individual sociologists make the discipline and profession possible; they provide the intelligence, skills, energy, effort and time required to produce, transmit, and apply the discipline, and to maintain the profession.

Collectivities: Departments are the major organizational units within the pro-

fession. Divisions of social science are becoming increasingly important because of the growth of two-year institutions. The financial resources made available to the profession through these collectivities largely support the production and transmission of the discipline.

Curriculum: The undergraduate curriculum is the major vehicle for transmitting the discipline to the larger society, and therefore, the major vehicle the profession has for providing service .to clients.

Students: Undergraduate students are the major clients of the profession and the major practitioners of the discipline upon graduation.

Life Skills: Life skills are "human relations and problem solving behaviors applied appropriately and responsibly to the management of one's life" (Conger, 1974:33, 47–48). They include the following areas of behavior: (1) self development, (2) family, (3) community, (4) job, and (5) leisure. The emphasis on life skills makes the "transfer of learning" a major objective of the curriculum.

CURRENT STATE OF CURRICULUM

The current state of the undergraduate curriculum is based on the assumption that a one-way flow of influence exists among the elements depicted in Conceptual Map 1. That is, the needs of the discipline determine (1) the organization of the profession, (2) the training of sociologists, (3) the organization of collectivities, (4) the components of the curriculum, and (5) what students need to learn.

What have been the consequences of this assumption for the undergraduate curriculum? The elements diagramed in Conceptual Map 1 suggest some of those consequences:

Discipline: The discipline has exhibited one of the characteristics associated with the growth of scientific fields by becoming more differentiated. Research has become narrower, more abstract, and more methodologically sophisticated. Scholarship—synthesis, interpretation, evaluation—has not kept pace, leaving the discipline fragmented.

---

[1] The idea for treating curriculum as a major social structure was suggested to me by Campbell et al., 1977.

Conceptual Map 1: Conceptual Map Underlying Current Undergraduate Curriculum
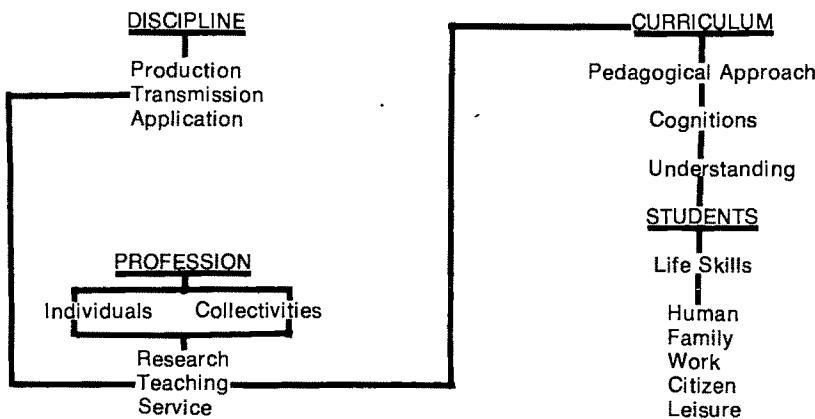
Profession: The organization of the profession, at least until recently, has been oriented toward the discipline. Status, recognition, and rewards have been given for the production of knowledge. Transmission and application of the knowledge have received less attention.

Individuals: The training of individuals has concentrated on the generation of knowledge through research in narrow specialties, while scholarship, dissemination, and application have been overlooked. Career advancement has depended on research and publication in certain journals, though the vast majority of sociologists have been employed under conditions severely restricting their abilities to do either.

Collectivities: Departments, and to a lesser extent divisions, have attempted to represent the discipline in decisions regarding hiring, tenure, promotions, and other rewards. The staffing task has become harder as the discipline has become more differentiated. To the extent that it was possible, departments have hired one faculty member per specialty, thereby reducing the possibilities for collegial relations rather than a community of scholars. Especially in four-year and graduate institutions the potential for collegial relations based on transmission and application of knowledge has not been developed because of the secondary importance attached to these functions.

Curriculum: Given the disciplinary orientation of the profession, the training provided in graduate departments, the career aspirations of individuals, and the staffing decisions of departments, the undergraduate curriculum could only be a smorgasbord of first courses in the numerous specialties that constitute the discipline. Under these circumstances the notion that sociology is an integrated, cumulative science has given way, along with some traditional notions associated with curriculum such as prerequisites and progression. Segmentation has been rationalized as breadth, and lack of coherence as liberal education.

Students: Students enrolled in sociology courses in large numbers during the last decade expecting to find a coherent body of knowledge that would help them solve the existential problems facing them and their society. Instead, they have found a selection of courses that has not given them a sense of progression as learners. In addition, they have been expected to perform a task which the profession has seemed unable to do—integrate the discipline.

Life Skills: Some students probably have acquired the life skills they needed through learning experiences in sociology courses. The number of such students probably is small because of the teaching techniques underlying the curriculum. Teachers have tended to provide students only with cognitions—theories, concepts, perspectives, facts—rather than emphasizing scholarship which couples cognitions with cognitive skills—methods of

Conceptual Map 2: Conceptual Map Underlying Proposed Undergraduate Curriculum
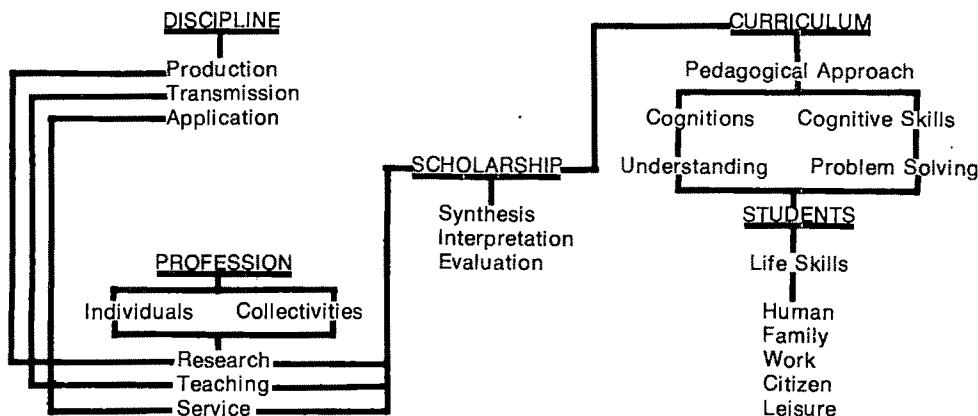
inquiry, observation, and problem solving—applicable to the problems facing society.[2]

The failure of the current undergraduate curriculum to help students acquire the life skills they need has started a reverse flow of influence among the elements in Conceptual Map 1. As students switch to other curricula, collectivities with less resources need fewer individuals in a shorter range of specialties. As the support of the profession declines, so does the ability of the profession to produce the discipline.

PROPOSED CURRICULUM

The curriculum proposed in this article rests on the assumption that an undergraduate curriculum requiring teachers to exercise scholarship while employing pedagogical techniques that emphasize inquiry, problem solving, and experiential learning will enhance all elements in our conceptual scheme. It also will add the dimensions that are missing in the framework underlying the current curriculum. (See Conceptual Map 2.)

The essential components of the curriculum are (1) teaching based on scholarship; (2) a pedagogical orientation that emphasizes inquiry, problem solving and experiential learning; (3) a sequence of seven general core courses; (4) a sequence of three specialized courses; and (5) the

___
[2] For a discussion of cognitions and cognitive skills see Posner and Rudnitsky (1978).

use of elective courses to add breadth or depth.

Obviously, this curriculum represents only one of the possible solutions to the problems addressed earlier in this article. It probably entails problems of its own; solutions to problems inevitably generate others!

FUNCTIONS OF SCHOLARSHIP

Scholarship occupies the central position in the proposed curriculum because it "fundamentally implies breadth, knowledge in depth, and a critical attitude toward that knowledge" (Kline, 1978:10). It is also reciprocally related research. "Without scholarship—the organization, explanation, interpretation, and criticism of research—the currently vast number of proliferating disciplines steadily gain in quantity as they lose in quality, vision, and effective use of the little in them that is worthwhile" (Kline, 1978:11). But without research—the empirical testing of the organizataion, explanation and interpretation of reality—scholarship would lose its connection with concrete reality and become philosophy.

Furthermore, scholarship is crucial because it (1) elucidates the otherwise "inscrutable results" contained in research papers; (2) synthesizes the relationship between works in different areas; (3) raises questions about the worth or direction of a particular specialization, thereby, keeping dissent alive, and (4) keeps in the forefront what the discipline as a whole is

trying to do, or should be doing (Kline 1978). In short, scholarship attempts to give meaning to an emerging body of knowledge.

It is interesting that a recent study of the federal investment in the production of social knowledge and its application to social problems has also called for the synthesis and integration of empirical findings, that is, scholarship (National Research Council, 1978a).

\

PEDAGOGICAL APPROACH

To be effective, however, scholarship must be coupled with pedagogical approaches that emphasize the development of cognitive skills as well as cognitions if students are to develop the life skills they need. Pedagogical approaches which meet these requirements have been suggested by Wilson (1977), Chickering (1977), and Baker (1975:1979).

Wilson believes that teaching, like research, should be "a disciplined search for reliable knowledge." Although he did not specifically mention "life skills" in his paper, the same principle must operate to foster them. Wilson further suggests that collaborative inquiry between teachers and students should be aimed at the pursuit of "a big question, with its complement of subsidiary questions."

Chickering argues for greater use of an experiential learning model, which is a cycle containing four stages:

1. Active participation in a concrete experience.
2. Observations of, and reflections on, that concrete experience.
3. Formation of abstract concepts and generalizations based on the observations and reflections.
4. Testing implications of the concepts and generalizations in a new concrete experience.

He writes: "Effective learning therefore has four ingredients that themselves call for four different abilities. The learners must be able to enter new experiences openly and fully without bias; they must be able to stand back from those experiences, observe them with some detachment, and reflect on their significance;

they must be able to develop a logic, a theory, a conceptual framework that gives some order to the observations; and they must be able to use those concepts to make decisions, to solve problems, to take action" (1977:18).

Baker advocates achieving cognitive and life skills through a "social awareness" teaching-learning strategy that deliberately compares and contrasts the perspectives of sociology with those of common sense and journalism. Through this approach Baker attempts to "reconcile the professor's specialized training with the student's general education needs." Baker hopes his strategy eventually will develop new capabilities for teaching sociology "as preparation for well-informed citizenship" (Baker, 1979).

GENERAL CORE COURSES

The sequence of seven general courses addresses the renewed emphasis on general education at the college level. It differs from previous core curricula in sociology in attempting to present the essence of the sociological perspective through courses focused on central questions in the discipline related to the development of life skills. The questions, themselves, could be used as course titles. Each course would draw on the breadth and depth of the discipline in an attempt to answer its central and subsidiary questions as fully as possible.

The sequencing of courses has been criticized, but it has been retained as a feature of this model for the following reasons: (1) An adequate grasp of the subject matter requires sequencing. Students must understand coordinated activities before they can fully understand the development of social identities that take place in coordinated activities. Basic understanding of coordinated activities and social identities is needed before students can learn how to acquire, evaluate, and act on knowledge about these activities and identities. Once these general problems are understood and the tools of inquiry are mastered, students are in a position to confront the enormous variety of human activities. At this point, students are ready to deal with the statics and

dynamics of coordinated activities. (2) Students should experience a sense of progress as they proceed through the curriculum. (3) Sequencing also may be useful in the development of the discipline; for some questions need to be answered before others can even be asked. Not everyone is likely to agree with the sequence below or even see the need for sequencing at all. As professionals we can agree to disagree on this matter.

Here are my suggestions for a sequence of courses, each presented in question form with a summary of the reasoning behind its formulation and its content.

1. *What makes the coordination of human activities necessary, possible, and problematic?* Neither individuals nor collectivities are self-sufficient; they depend on other individuals and other collectivities for their existence. This course will address such questions as: In what ways are individuals and collectivities dependent on other individuals and collectivities? Is the interdependence between individuals different from the interdependence between collectivities? Have the social mechanisms created by humans to handle the problem of interdependence increased or decreased the problem? What general conditions are required by coordinated actions? What are the perennial problems associated with coordinated actions, and why haven't they been solved?

In searching for answers to these questions, the course will draw on knowledge about social systems, communication processes, power relationships, systems of rewards and punishments, role structures, systems of stratification, and normative systems.

2. *How do individuals and collectivities develop social identities and a sense of self?* Individuals (biological beings) and collectivities (social mechanisms) have varying potentials for development. This course will address such questions as: How do biological potentials influence social identities and a sense of self in individuals? How do financial, human, and cultural resources influence social identities and a sense of self developed by collectivities? How do social identities and a sense of self develop? What are the

major influences on social identities and a sense of self? Do social identities change? Can they be lost?

In searching for answers to these questions, the course will draw on knowledge about personality and social structure, symbolic interaction, socialization, perception, life cycles, behaviorism, cognitive consistency, self-image, goals, mass communication, and belief systems.

3. *How is knowledge about coordinated human activities, social identities, and a sense of self acquired, evaluated, and acted on?* All individuals and collectivities need reliable knowledge on which to base their actions in coordinated activities and to develop a social identity, and a sense of self. This course will address such questions as: Can the various bases of knowledge be ranked in terms of reliability? How does one determine what sources of information exist in relation to a specific problem? How does one go about retrieving the information? How should problems be approached? How should a study be conducted? How can a study be evaluated? If we have the information, what problems will be encountered in applying it?

In searching for answers to these questions, the course will draw on knowledge about defense mechanisms that may interfere with perception, the sociology of knowledge, information search and retrieval techniques, decision-making and problem definition, quantitative and qualitative research methods, policy research, and applied or clinical sociology.

4. *What are the common and unique problems confronting the numerous forms of coordinated activity developed by humans?* Humans have created an enormous number of coordinated activities to handle the problem of interdependence. This course will address such questions as: Is there any way to order the numerous forms of coordinated activity created by humans? Why is the same problem addressed by so many different forms of coordination? Why are different problems addressed by the same form of coordination? What aspects of the problem determine what form of coordination will be used?

In searching for answers to these ques-

tions, the course will draw on knowledge about social institutions, types of coordinated activity, families, and formal and informal organization.

5. *What influence do the characteristics of populations have on the various forms of coordinated activity developed by humans?* All coordinated activities involve two or more individuals or collectivities. The demographic characteristics of a population therefore should influence the form of the coordinated activities.

This course will address such questions as: How do the various characteristics of populations affect coordinated activities? Are some characteristics of populations more supportive of coordinated activities than others? What combinations of characteristics create what kind of problems for coordinated activities?

In searching for answers to these questions the course will draw on knowledge about social demography, population growth and decline, immigration, migration, race relations, stratification, ethnic and occupational cultures, collective behavior, and social movements.

6. *Why don't forms of coordinated activity change easily?* All coordinated actions face the problems of prediction and stability. For a coordinated action to succeed, each participant must be able to predict the behavior of the other participants in the action. Prediction rests on conformity to expectations. Expectations need to remain stable over time so that each participant can learn them.

This couse will address such questions as: What enables a participant to predict the behavior of the other participants in a coordinated action? Why do participants conform to the behaviors expected of them? How does one learn the expected behaviors? What other factors effect stability of coordinated actions?

In searching for answers to these questions, the course will draw on knowledge about conformity, authority, reference groups, roles, norms, rewards, punishment, self-esteem, consensus, values, beliefs, vested interests, socialization, and resources.

7. *Why do forms of coordinated activities change?* All coordinated activities face the problems of uncertainty and change. This course will address such questions as: What are the uncertainties facing coordinated activities? How are they handled? What causes changes in coordinated activities? What problems does change create for coordinated activities? How does change improve coordinated activities? How does change destroy coordinated activities? How can coordinated activities change, but remain the same?

In searching for answers to these questions, the course will draw on knowledge about social change, conflict, deviance, science, resources, environment, social movements, technology, social innovations, and populations.

SPECIALIZED COURSES

The sequence of three specialized courses addresses the increasing interest students have exhibited in vocational training. The general core courses present information and skills useful in living and in any occupation; the specialized courses provide depth in an area relevant to specific occupations or a limited range of occupations. They could provide majors in sociology with a solid background for pursuing advanced training for other occupations. There could be a sequence in the sociology of law for students planning to become lawyers; a sequence in medical sociology for future doctors; a sequence in marriage and the family for counselors; a sequence in the sociology of religion for aspiring ministers; or a sequence in theory and methodology for future sociologists.

Other sequences, probably augmented by electives, could help sociology majors with bachelor degrees to find jobs. A sequence organized around the sociology of work, occupations and professions, plus complex and formal organizations, might help graduates land jobs in personnel, management, and counseling. A sequence organized around urban, environmental and community issues, ethnic and minority relations, political, collective and social movements might be useful background for careers in planning, politics, government, and journalism. A sequence organized around research techniques and computer science could facilitate em-

ployment in various kinds of research and the management and analysis of data.

SERVING NON-MAJORS

The proposed curriculum is aimed at students who will take a 10-course major in sociology, but it also can serve the non-majors who sustain enrollment levels in sociology courses as the number of majors declines. Students seeking to fulfill degree requirements in the social sciences may find the first three general courses more appealing than the traditional offerings. Students majoring in other disci-. plines could be allowed to enter any of the courses in the curriculum providing they could demonstrate appropriate background. Two-year institutions could concentrate on the general core courses.

EXPECTED OUTCOMES

The proposed undergraduate curriculum is expected to have the following outcomes for each of the elements in our framework if it is adequately implemented:

Discipline: The discipline should benefit because the curriculum focuses on central questions in the discipline and calls for the scholarship needed to integrate the various paradigms and discrete research findings. Teachers and students would join researchers in producing the discipline.

Profession: The profession should change its social organization so that it supports the generation, transmission, and application of the discipline. In so doing, it will provide its clients with the services it is capable of providing while, at the same time, laying claim to the support of those many teachers and practitioners who do not presently participate in the profession.

Individual: Individuals who have no inclination or opportunity to do empirical research will find other recognized avenues for professional development to open up career opportunities and reinforce their self-identification as sociologists. The general core courses provide an opportu-

nity to exercise scholarship; the specialized courses provide an opportunity to delve deeply into a specialty.

Collectivities: Departments and some divisions will find it easier to represent the discipline to provide services that attract clients, and to defend the distribution of rewards on a variety of grounds. The commonality and interdependence promoted by the proposed curriculum may motivate the aggregate of individuals to become a community of scholars, whether their primary interests are in research, teaching, or application.

Students: Students will experience a sense of progress and development as they acquire understanding and information coupled with cognitive skills. Rather than having the responsibility for integrating the discipline unfairly thrust upon them, they will participate in a collaborative effort to achieve integration.

Life Skills: Having acquired the social identities of learner and problem-solver, students should be more confident and competent to cope with their own problems as they take up their roles in a variety of social structures. Perhaps then, the degree of support required for the further development of the discipline will be forthcoming; for the students will be sure to recognize the value of their undergraduate training in sociology.

CONCLUSION

This article attempts to demonstrate the central importance of the undergraduate curriculum for the discipline and the profession of sociology, in order to focus needed attention on the current state of the curriculum, which presently is viewed as dysfunctional for the discipline and the profession. To make the case it was necessary to range far beyond what normally are considered curriculum matters, and point to the interdependency that exists between the curriculum, the discipline, and the profession with its constituent units—individuals and collectivities.

This article also proposes an undergraduate curriculum calling for scholarship from teachers that would assist

students to develop the life skills they need and promote the development of the discipline as an integrated body of knowledge. The proposed curriculum undoubtedly has flaws. My colleagues no doubt will identify these problems and offer their own curriculum proposals for consideration.

REFERENCES

Baker, Paul J.
  1975   "Social awareness project." Teaching Sociology 3:74–80.
  1979   "Teaching/learning strategy accents citizenship role." ASA Footnotes, January, 1979.
Bates, Alan P. and Sue Titus Reid
  1971   "The sociology major in accredited colleges and universities." The American Sociologist 6:243–249.
Campbell, Frederick L., David Kaufman, C. Jack Gilchrist, R. Kelly Hancock, John Klobas, Ann Sundgren
  1977   "Experimenting with curricular design: Some problems and possibilities." Teaching Sociology 5:49–64.
Chickering, Arthur W.
  1977   Experience and Learning: An Introduction to Experiential Learning. New Rochelle, NY: Change Magazine Press.
Conger, D. Stuart
  1974   Social Inventions. Prince Albert, Canada: Information Canada.
Curriculum Task Group, ASA Projects on Teaching Undergraduate Sociology
  1977   Sociology Curriculum. Sharon McPherron (ed.), Teaching Sociology 5:3–120.

Davis, Ethelyn, Charles Goldsmid, Wilhelmina Perry
  1977   Teaching Sociology: A Bibliography. Washington D.C.: ASA Teaching Resources Center.
Gibbs, Jack P.
  1979   "The elites can do without us." The American Sociologist 14:79–84.
Kline, Morris
  1978   "Mathematics and the dilemma of university education." The Mathematical Intelligencer 1:5–14.
National Research Council
  1978   The Federal Investment in Knowledge of Social Problems. Summary Report of the Study Project on Social Research and Development. Washington, D.C.: National Academy of Sciences.
  1978a Knowledge and Policy: The Uncertain Connection. Laurence E. Lynn, Jr. (ed.), Report of the Study Project on Social Research and Development, Vol. 5. Washington, D.C.: National Academy of Sciences.
Posner, George J. and Alan N. Rudnitsky
  1978   Course Design: A Guide to Curriculum Development for Teachers. New York: Longman.
Reid, Sue Titus and Alan P. Bates
  1971   "Undergraduate sociology programs in accredited colleges and universities." The American Sociologist 6:165–175.
Stryker, Sheldon
  1979   "Stryker: Identity crisis in sociology calls for common vision of relevance." ASA Footnotes, August, 1979.
Wilson, Everett K.
  1977   "Sociology: Scholarly discipline or profession?" Paper presented before ASA Section on Undergraduate Education, ASA Annual Meeting.

CAREERS AND THE UNDERGRADUATE CURRICULUM: AN INTEGRATED PROGRAM*

CHARLES S. GREEN, III, HADLEY G. KLUG, LANNY A. NEIDER, AND RICHARD G. SALEM
University of Wisconsin-Whitewater

"You can't do a damned thing with a Bachelor's in Sociology—or can you?" This question is receiving increased attention among faculty interested in providing students with more than vague statements about the relevance of sociology. We contend that the prospects for B.A./B.S. sociologists finding career opportunities in nontraditional areas of employment can be enhanced through a strategy that incorporates employment and employers into the educational process while maintaining the integrity of a strong liberal arts program. Outlined here are the four interrelated parts of a program our department has developed and implemented. A Handbook for Sociology Students *is designed to help students clarify and develop career objectives. We next describe our "career path" counseling program, which recommends specific curricula appropriate to each of twenty-one career options. We then summarize our experience with an internship program. Lastly, we review our career seminar, which helps students develop skills important in the job search.*

It should be no news to readers of this journal that sociologists as individuals recently have been forced to seek nontraditional employment, and that the profession itself increasingly has become concerned with opening up non-academic job opportunities (Panian and DeFleur, 1975; Foote, 1974; Morrissey and Stedman, 1977; Gelfand, 1975; Kay, 1978). But much of the concern of the profession has been with expanding non-academic opportunities for those with masters and doctorates rather than for those having only undergraduate degrees (Morrissey and Stedman, 1977; Demerath, 1971; Ferris, 1968; Friedman and Olson, 1973; McGinnis and Solomon, 1973; Cartter, 1978; Kay, 1978). Yet undergraduate sociology majors are far more numerous and are as troubled—if not more so—over the vocational uses of their degrees as graduate degree holders (Dayton, 1979;

Schultz, 1974; Terry, 1979; Fleming and Francis, 1977; Stevens and Todd, 1977).

We became especially concerned about undergraduates after reviewing the results of a survey completed in mid-1976 of the undergraduate majors in our department who had received their degrees in the years 1971–1975. We found that 27% of our graduates took their present jobs either as an interim one "until something better comes along," or because they had "nothing else from which to choose." Fully 25% reported "lots of trouble" finding work and an additional 33% reported they had had "some trouble" finding work. Over half of our graduates thought our department or the university should have prepared them better for the job market.

A representative sample of comments to our open-ended question on how we could have better prepared students for the job market reveals just how severe their criticisms were:

"More practical knowledge of the working world. Facts not fantasies!. . . ."

"Remind you that your education, if for nothing else, is an experience for learning's sake and don't expect to find a job with it!"

". . . Tell me that since I don't have much experience, I'll have to settle for a lower paying job to get experience. Offer an internship program."

"I asked no help from anybody and got none. I have wasted 5 years and now I am

going to begin looking for jobs in areas of interest to me."

"Truthful answers regarding the real situation of the job market. . . ."

". . . You people did nothing for me, no interest in me or my education."

One possible response to these sentiments would have been to modify both the content and variety of our courses, making them more practical (e.g., Handcuffs 101) and thereby more immediately relevant. Many of our graduates urged us to do just that. Their position, apparently, would be supported by a number of sociologists.

Indeed it is apparent that many sociology departments already have decided that course content and the types of courses offered should be changed in order to attract and market sociology majors. The evidence for this is the proliferation both of courses and programs in such "applied" areas as criminology and health and welfare services, as well as the dilution of course requirements (Hunter, 1979; Bates and Reid, 1971; Gates, 1969; Shin, 1975; Hansen et al., 1976). In the area of criminology, at least, the intellectual quality of many of the courses and programs has been severely criticized (Sherman, 1978). We are of the opinion that, as a matter of principle and even at the risk of reduced enrollments, such changes in course content, types of offerings, and rigor should be avoided. We see our goal as instilling in students sociology's unique perspective on human affairs, a goal to be implemented through an intellectually demanding curriculum, starting, as D'Antonio (1978) has urged, with the introductory course. To adopt any other position is to perpetuate that mindlessness of the powerful that C. Wright Mills called "the higher immorality."

The assumption that course content and course offerings must be revised drastically in order to make our graduates marketable needs to be challenged on practical grounds as well. If it is true, as Rhoades (1974) has argued, that most non-academic employers do not understand what sociologists are and what we do, it seems the height of folly to adapt our curriculum to their misunderstanding. Since such misunderstanding is likely to be caused by employers' own lack of education and/or lack of contact with employees educated in sociology, the best strategy for the profession to adopt is to change employers' knowledge and attitudes rather than changing ourselves. Such a strategy could be effected most simply and quickly by placing more sociologists in internships and in regular employment with non-academic employers (Brown and Allen, 1979). In short, our strategy must become subversion from within!

Moreover, little "hard" evidence has been produced to show that employers even in business are unwilling to employ undergraduates who have received a general liberal arts education. Indeed, there is "hard" evidence to the contrary. Based on their survey of North Carolina manufacturing firms, Reiser and Maiolo (1979) conclude that "The fact that 6.2 percent of the companies indicated that they currently employed a sociologist with a B.A. degree seems to say that these firms are open to our field. It also forbodes well that a slightly higher percentage of companies foresee a future need for [more] sociologists than they currently employ." Furthermore, if one examines the undergraduate background of the chief executive officers of the five hundred largest industrial corporations, one finds that 75% of them have undergraduate degrees in the liberal arts. Since liberal arts degrees constitute 71% of the degrees awarded in the three fields from which the largest industrial corporations draw virtually all their chief executives (liberal arts, business, and engineering), it is clear that liberal arts majors are *over* represented in the ranks of higher industrial management.[1] It would appear, therefore, that there is little need to make our curriculum less rigorous or more "practical."

We have implemented what we believe is a viable alternative to tinkering with the

---

[1] Data on chief executive officers were gathered by Robert Fritz for a term paper. Of the 500 chief executive officers, 68 were not listed in *Who's Who in America*. For another 36 no information was available on the major field of their undergraduate degrees, if any. Thus the results reported in this paper are based on 396 or 79.2% of the chief executive officers of the 500 largest industrial firms.

traditional curriculum. We have retained a demanding curriculum for all majors, which includes within its 33 semester hours a required introductory course, two required courses in methodology, and a required course in theory. But we have supplemented our curriculum with a career-oriented program consisting of four interrelated parts. The following discussion reviews this program in detail. We first will consider the *Handbook for Sociology Students;* next, our "Career Path" counseling and our internship course; and last, our Seminar in Career Development.

A HANDBOOK FOR SOCIOLOGY STUDENTS.

The *Handbook for Sociology Students* is designed to put into the hands of all students an integrated source of information on questions pertaining to curriculum, potential career opportunities (including graduate education), and faculty interests and activities. It begins with a short introduction designed to acquaint the student with the unique perspective that sociology has to offer. Next, the *Handbook* presents the major and minor degree requirements and provides descriptions of the department's course offerings and their content.

The second section of the *Handbook* suggests sources of career information as well as a series of career planning tactics. Also included in this section is information on what may be required for further study at the graduate level and a few of the preparatory tasks that must be accomplished prior to entrance into a graduate program. This section ends with the presentation of a number of potential career areas including business, personnel management, law enforcement, urban planning, social services delivery, and government employment. Examples are given in each of these areas of specific jobs that may be open to an individual with an undergraduate sociology major or minor.

Lastly, the *Handbook* provides information on the faculty in our department. A unique feature of this section is a "professional biography" for each faculty member. These were written to suggest the diversity and depth of both academic

and non-academic experiences available to students.

CAREER PATHS IN SOCIOLOGY

Despite the development of the *Handbook* two years ago, we had two continuing problems in trying to help students clarify their career objectives and develop an appropriate program of study. Although the *Handbook* listed career options, it did not make plain to the student exactly what any of these options entailed. As well, it was unclear even to our faculty how a program of study at the university might be constructed to prepare a student for any particular career. During a number of informal discussions there emerged the idea of develoing a series of "career paths" along with a suggested program of courses within the sociology major, the minor, and "electives" for each of these "paths." A paper, "Career Paths in Sociology," was prepared by one of the authors; it describes twenty-one career options with a suggested academic program for each. In essence, this paper provides the framework for *combining* career counseling with academic advising.[2]

Most crucially, however, the "paths" go beyond the usual areas of social welfare, police, and corrections. The paper groups the twenty-one alternatives into three areas: (1) direct human services; (2) business corporations; and (3) administration and planning/Federal and State government. There are ten paths in the *direct human services* area ranging from "social welfare casework" through "juvenile probation, residential and group home treatment," "vocational counseling," and "police." Next, there are six paths in the *business corporations* area including "general management," "personnel management," and "general marketing." Lastly, there are five paths in the *administration and planning/Federal and State government* area including "management/administration" and "research/planning analysis." Prepara-

---

[2] We expect to incorporate this paper into the second edition of the *Handbook.*

tion for careers in specialized areas of business and administration/management (in the private as well as the public sector) recently have become viable options through the adoption by our College of Letters and Sciences of a "professional minor in business."[3] This allows the sociology major to choose from a series of business emphases in preparation for employment in business corporations or governmental agencies which seek individuals with broad liberal arts backgrounds but with some course work appropriate to their specialized needs.

Two widely divergent career paths described in the "career path paper" are reproduced below:

1. *juvenile probation, residential and group home treatment*

   Local counties employ graduates with degrees in social welfare, sociology, or psychology as juvenile probation officers. Also, there are larger numbers of positions available as counselors in private, county, and state operated group homes and treatment centers. The work of the juvenile probation officer centers both on treatment and control of juveniles referred by police (or schools), or those declared "beyond parental control" by their parents. Specifically, the work of the probation officer involves examination of family and child problems, recommendations of disposition plans to the court, family counseling, acting

as a referral agent to other agencies, and monitoring of the juveniles' behavior in the family, at school, and in treatment settings. On the other hand the group home or treatment center counselor is most often concerned with the establishment of acceptable behavior patterns by youth at the group home or center. The counselor leads group counseling sessions, plans activities, deals with youth-family relations, and seeks to establish an environment of trust and affection in which the young person can develop an acceptable level of self control.

RECOMMENDED CURRICULUM FOR JUVENILE PROBATION, RESIDENTIAL, AND GROUP HOME TREATMENT

sociology major to include:
(880276) Introduction to Criminology
(880459) Sociology of Minorities
(880370) Juvenile Delinquency
(880492/493) Applied Sociology (in this course students are placed in an agency for practical experience)

psychology minor to include:
(840345) Behavior Disorders
(840446) Introduction to Clinical Psychology
(840486) Interview and Psychotherapy Techniques

or

if these courses not taken in psychology then:
(860102) Introduction to Social Welfare
(860271) Social Welfare Methods I
(860372) Social Welfare MethodsII

2. *management/administration*

   There are substantial opportunities for graduates with B.A. degrees in sociology, political science, and public administration to obtain employment with the state of Wisconsin and the Federal government (as well as in municipal government) in the area of management/administration. These opportunities exist in many areas, from personnel management through property management and contract administration. The state and federal agencies in which employment is available encompass a great range of specific activities, from criminal justice and corrections through agriculture, transportation,

---

[3] From the standpoint of the politics of educational change, it is interesting to note that the idea of a minor in business was suggested by one of the authors to the Dean of our College. It later became apparent that our Dean had spoken to others (including the Dean of the College of Business and Economics) about this suggestion for he subsequently formed an *ad hoc* committee to explore the suggestion more formally. Another of the authors volunteered for this committee. Both our suggestion of the business minor and our representation on the committee were far from chance happenings. We had in fact decided several months earlier that such a minor would benefit our College and Department more than it would the College of Business and Economics. The latter College assumed the new minor would have little impact on them. Consequently agreement over and implementation of the minor proceeded rather rapidly.

and consumer protection. Moreover, student internships (some of which are paid) and post-graduate traineeships (all of which are paid) are readily available.

sociology major to include:
(880280) Sociology of the Future
(880380) Bureaucracy and Democracy
(880492/493) Applied Sociology (in this course students are placed in a government agency most compatible with their specific career goals)

political science, public administration, individualized general business or general management minor to include some of the following:
(820141) Introduction to American Government and Politics
(820320) Public Policy and Administration
(250211) Data Processing*
(250310) Management Concepts
(210244) Accounting Concepts
(230356) Public Finance*

INTERNSHIP PROGRAM

An integral part of our effort to help students clarify career goals and gain specialized experiences appropriate to these goals is our internship placement program. Applied Sociology (as our program is known) is designed as an elective for juniors and seniors who are majoring or minoring in sociology. Course credit (which is based on a formula according to hours worked) is given for approved placements in numerous agencies and organizations. It was initiated during the summer semester of 1976 and has had interns participating every semester since then. During this period a total of 48 placements have been made in the following areas: police and adult corrections (13), juvenile corrections and group homes (12), counseling (including vocational counseling)/social services (16), research/planning (4), administration/

---

*Course (250211) has Math (760143) as a prerequisite and course (230356) has Economics (230211) and (230212) as prerequisites.

management (2), and senior citizens centers (1).

As might be expected, we have found that one of the most difficult aspects of an internship program is student indecision about the kind of placement they desire, as well as the search for an organization or agency willing to take the time necessary to work with students. Luckily many agencies see student interns as extra help at no expense (although on a number of occasions students actually have been paid). Students usually have at least a general idea of what kind of internship experience they would like (e.g., "working with people" vs. research/planning), but it often is necessary to suggest concrete alternatives as well as specify what sorts of tasks and activities are expected. In most cases several alternatives are evaluated for each student through phone calls or visits by the coordinator. Approximately half of the placements are with agencies or departments that have not worked with our program before. Matching student to job is purely subjective. Since the coordinator usually has had some contact with the student through course work and at least a fairly lenghty interview, he tries to take into account such factors as verbal ability, personal assertiveness, and writing ability. Past grades are not taken into account, and, most important, no student has been refused an internship. We see it as a right—not a privilege.

Probably the most time consuming activity related to the program has been the effort to develop a network of contacts at agencies and organizations. The network can be said to have been painstakingly developed using techniques that range from simply calling phone numbers for an agency in the directory and being referred from one person to the next until some results are achieved, through contacting former students who now are employed. Contact with agencies is maintained through visiting supervisors for the purpose of evaluating student interns, lunching with past or current supervisors, or—on occasion—having agency people as guest speakers in class. Some placements also are used on a continuing basis. Often the program coordinator combines

student evaluation and lunch with one or more agency personnel. On occasion he even has been known to conclude an "evaluation" visit with a second, and more lengthy, visit to a local bar (along with the agency supervisor, of course).

In general, wide opportunities for career related experience have been provided, ranging from such direct human services as police work and counseling (both individual and group) to evaluation research. Every agency has been willing to provide opportunities for students to become involved in a substantial range of activities that may occur at a particular agency. For example, one student who was placed with the city of Madison (Wisconsin) Police Department, not only patrolled with police, but worked with the community relations, alcohol/drug abuse, detective, and youth aid divisions. Moreover, shortly after her internship she was one of sixteen new officers hired by the Madison Police Department from over 350 applicants.

Overall, supervisors have been cooperative in providing meaningful activities for students. There have been no placements where students have been ignored. In only one situation has agency personnel felt substantial dissatisfaction with the performance of a student. When the internship coordinator learned of this and discussed the problem (which could be described as agency workers' perception of a "know it all attitude" on the part of the student) with the supervisor, it was resolved quickly.

No student has expressed dissatisfaction with the experiences gained at a particular placement. On the contrary, almost uniformly they have been very positive. Nevertheless, whether placements have led to specific careers or at least "career clarification" is still not known. In most cases it will be necessary to consider career consequences the next time we survey our graduates. At this point our impression is that consequences vary widely. For example, one student's placement at the Rock County (Wisconsin) Office of Juvenile Probation led directly to his being hired as a full-time juvenile probation officer. On the other hand another's placement at the same agency led to the change of her major from "sociology" to "social welfare."

A SEMINAR IN CAREER DEVELOPMENT

The career seminar meets one to two hours per week for a semester and is offered as an elective to all sociology majors with junior or senior standing. Students receive one semester hour credit for the seminar; however, this credit is not applicable to major or minor requirements.

An outline of the topics covered in the seminar may be found in Figure 1. The objectives of the seminar include providing students with the information and skills necessary for selecting a graduate program, finding and obtaining jobs, and managing career contingencies. In achieving these objectives traditional discussion methods and extensive out of class reading assignments are supplemented by a series of exercises. These exercises are designed to force students to develop through actual experience the skills and knowledge of sources they need to find and obtain jobs for themselves without having to rely on the often poor services offered by employment agencies, college placement services, consultants, etc.

One such exercise, "The Quick Job Hunting Map," is assigned from Bolles' *What Color Is Your Parachute?* (1978). This exercise requires students to develop an inventory of their values, skills, and interests—an important prerequisite to finding satisfying and rewarding work. A second exercise, "Where Do You Want to Do It?," requires students to use the reference section of the library to find standard sources of information on: the future of employment, types of jobs available, and salary ranges in various sectors of the economy; the names of, and background information about, executives; the products or services offered by various businesses and government agencies; climatological, demographic, social, economic, and political data on cities, states, and regions. Using these sources students must find a minimum of three employers likely to have jobs of interest to them in

Figure 1. Outline for the Seminar in Career
Development

Session	Topics
1	A. Introduction 1. Goals of the seminar 2. The syllabus: a preview of things to come 3. The relevance of sociology and of the liberal arts degree
2 & 3	B. Not Just Any Job Will Do 1. What can you do, what do you enjoy doing? 2. A first approximation: what jobs are available for someone with your abilities, likes, and dislikes? *Exercise*: The Quick Job Hunting Map
4	C. Do You Need or Want An Advanced Degree?
	D. Where's the Action?
5	1. Labor markets and locating information about job openings
6	2. Where do you want to do it? a. Picking the "right" employer b. Picking the "right" location *Exercise*: Where Do You Want to Do It?
7	E. Finding an Appropriate Graduate Program *Exercise*: Picking a Graduate Program
8	F. Preparing Oneself for the Battle 1. How to get to know your future employer's problems 2. How to invent jobs that no one but you can fill
9	3. The presentation of self in writing *Exercise*: Application letters *Exercise*: Resumes
10	4. Putting your foot in the boss' door: how to get around secretaries and other gatekeepers *The Chutzpah Exercise*
11 & 12	5. Putting up a good front once you're in the door a. Tests b. Interviews c. Other hassles: discrimination, etc. *Exercise*: Round One Interview *Exercise*: Round Two Interview
	G. Decisions, Decisions, Decisions
13	1. Should you be so lucky as to get more than one offer, which one should you pick?
14	2. Career management a. Planning for the long term b. The uncertainties of success
15	3. Ethical dilemmas at work: can you avoid becoming a white collar crook?
16	4. Plan now for retirement?!

each of three cities whose characteristics they find attractive. Another exercise, "Picking a Graduate Program," requires students to find and use standard sources of information in order to select three graduate programs that would provide the education or training relevant to the sorts of jobs in which they are interested. The fourth and fifth exercises, drawn from Figgins' *Techniques of Job Search* (1976), require students to write a resume and a letter of application and to correct examples of faulty letters provided in Figgins' book. "The Chutzpah Exercise" requires students to obtain personal information, preferably through an interview, about a national figure such as Jane Fonda or about a local notable such as the Mayor of Milwaukee. Suitable rewards (e.g., beer) are provided to those few who actually manage to obtain interviews, and much is learned by the discussion of successes and failures.

Two rounds of video-taped simulated interviews require students to put much of what they have already learned into practice. Each student is given a description of a real job from a newspaper or other source and urged to find out as much as possible about the job, the company or agency, and supervisor of that job. In further preparation students must compose an application letter and a resumé specifically designed for that job. The first round of interviews is intended to expose each student to a fairly structured and low stress interview situation. A student first is interviewed by another student playing the role of "interviewer," i.e., the supervisor or recruiter. When the interview is complete, the "interviewer" provides the "interviewee" with an evaluation on a check-off rating form. The students then switch roles. Later each student reviews his or her video-tape, evaluates it, and compares the evaluation with that provided by the "interviewer." In the second round students are assigned new partners. "Interviewers" are expected to conduct an unstructured and highly stressful interview. The seminar director reviews at least one video-tape from each student and provides additional constructive comments.

Seminar attendance counts 12% toward the final course grade while each of the eight exercises counts 11%. Each student's exercise write-up is carefully criticized and assigned a grade. However, students are permitted to revise and resubmit their work for a higher grade. This "revision provision" encourages them to learn from, rather than be penalized for, mistakes.

SUMMARY AND CONCLUSION

In attempting to attract undergraduate majors and expand the job market for them, many have eloquently stressed the need for imaginative developments within the sociology curriculum. Others stress the importance of pedagogical skill and technique in order to advance the ideals of traditional sociology in a new order. Yet the chance of sociologists doing sociology depends to a very considerable extent upon the public's, and especially employers', understanding of, and familiarity with, sociology. Few departments have sought to approach this problem of fulfilling sociology's promise by means of a program such as ours that incorporates employment and employers into the learning process, while maintaining the integrity of a traditional curriculum.

No one part of our program is unique. Other departments have developed handbooks for their majors (Vaughan and Cech, 1977). Internships hardly are innovative either (Lees, 1977). Other departments recently have developed "career paths" or "custom tracks" (Lees, 1976; McIntosh, 1977; Terry, 1979), though we may have gone further than most in combining academic counseling and career advising. We are perhaps unique in offering a career seminar through our department rather than through the University's placement service, but career seminars in general have a long history.

What, then, is unique about our program? We believe the program is unique in integrating all four parts into what is a traditional, albeit fairly rigorous, curriculum. More important, developing the program required us to rethink and clarify

the purposes of the traditional undergraduate liberal arts sociology curriculum, and in so doing, to become aware of the necessity to include within its boundaries career planning and job getting. We are convinced we have found a viable answer to those perennial questions: what is sociology?, what is the appropriate place for sociology in the liberal arts tradition?, does sociology have a valid contribution to make as preparation for employment, as well as for graduate and professional schools? In addition, we think such a program addresses the need both to inform students and especially employers of what sociology is.

It is far too early to provide any definitive evaluation of our program. We do know that the reactions of our students to the internships, the handbook, the career seminar, and career path counseling have been highly favorable. Moreover, our class enrollments, majors and minors, have been increasing, whereas within our College of Letters and Sciences only two other departments, both vocationally oriented (Journalism and Social Welfare), have experienced such increases; all other departments have experienced decreases. The Colleges of Arts and Education have experienced decreases as well. Only the departments within the College of Business and Economics have grown markedly faster than our department. Through surveys of our graduates and employers, we expect to be able to provide more conclusive evaluation data.[4]

In the future, as earlier discussion suggests, we intend to emphasize strongly careers and internship placements in research, planning, public administration, and business management. Although this effort is partially spurred by what seems to be a favorable job market in these areas,. we believe that sociology B.A.s in such positions can have a progressive impact on decision-making within orga-

---

[4] We are also now in the process of designing a survey to focus on employers' criteria for employee selection for those positions for which we feel sociology graduates have appropriate academic preparation. Moreover, we also will seek to ascertain the "image" employers have of liberal arts graduates as persons and as potential employees.

nizational structures (Ebersole, 1979; Miner, 1971). Further, we see sociology student interns and employees educated in sociology as eventually influencing organizations to evaluate more positively the usefulness of the sociology major as career preparation. The future includes as well an attempt to spread among high school students, their parents, and their counselors the message that you can indeed do something with a liberal arts degree—even one in sociology!

REFERENCES

Bates, Alan P. and Sue Titus Reid
1971 "The sociology major in accredited colleges and universities." The American Sociologist 6:243–249.
Bolles, Richard Nelson
1978 What Color Is Your Parachute? Berkeley, CA: Ten Speed Press.
Brown, William R. and William A. Allen
1979 "Three urgent needs of sociology." The Southern Sociologist 10:18–20.
Cartter, Allan M.
1976 Ph.D.s and the Academic Labor Market. New York: McGraw-Hill.
D'Antonio, William V.
1978 "Teaching sociology: An impossible dream?" Presented at the annual meetings of the American Sociological Association.
Dayton, Peter
1979 "Point of view." The Chronicle of Higher Education 18 (May 29):80.
Demerath, Jay
1971 "Notes on a nervous job market." The American Sociologist 6:187–188.
Ebersole, Mark C.
1979 "Why the liberal arts will survive." The Chronicle of Higher Education 18 (May 21):48.
Ferris, Abbot L.
1968 "Forecasting the supply and demand of sociologists." The American Sociologist 3:225–233.
Figgins, Ross
1976 Techniques of Job Search. San Francisco: Canfield Press.
Fleming, P. L. and R. G. Francis
1977 "Careers or jobs: Notes from sociology alumni." Presented at the annual meetings of the Southern Sociological Society.
Foote, Nelson N.
1974 "Putting sociologists to work." The American Sociologist 9:124–134.
Friedman, Norman and Frances Olson
1973 "Post-degree careers of state college terminal master's recipients in sociology." The American Sociologist 8:119–127.
Gates, D.
1969 "Sociology in small United States liberal

arts colleges." The American Sociologist 4:324–330.
Gelfand, Donald E.
1975 "The challenge of applied sociology." The American Sociologist 10:13–18.
Hansen, Roy A., Daniel J. Stivers, and Roy G. Francis
1976 "Student power and academic destandardization." College Student Journal 10:217–225.
Hunter, Joan (ed.)
1979 Peterson's Annual Guide to Undergraduate Study, Vol. 1. Princeton, NJ: Peterson's Guides.
Kay, Paul
1978 "The myth of nonacademic employment: Observations on the growth of an ideology. Comments. Rejoinder." The American Sociologist 13:216–219.
Lees, Mary W.
1976 "Career planning for sociology undergraduates." Presented at the annual meetings of the Midwest Sociological Society.
1977 "Applied career exploration for sociology undergraduates." Presented at the annual meetings of the Midwest Sociological Society.
McGinnis, Robert and Louise Solomon
1973 "Employment prospects for Ph.D. sociologists during the seventies." The American Sociologist 8:57–63.
McIntosh, J. R.
1977 "Clients and customers: The academic marketplace." Presented at the annual meetings of the Eastern Sociological Society.
Miner, John B.
1971 "Changes in student attitudes toward bureaucratic role prescriptions during the 1960s." Administrative Science Quarterly 16:351–364.
Morrissey, Joseph P. and Henry J. Steadman
1977 "Practice and perish?: Some overlooked career contingencies for sociologists in nonacademic settings." The American Sociologist 12:154–162.
Panian, Sharon and Melvin L. DeFleur
1975 Sociologists in Non-Academic Employment. Washington, D.C.: The American Sociological Association.
Reiser, Christa and John Maiolo
1979 "Sociologists in private enterprise: The job market for non-Ph.D. sociologists." The Southern Sociologist 10:16–18.
Rhoades, Lawrence J.
1974 "Expanding the labor market for sociologists." Footnotes 2 (February):2.
Schultz, C. C.
1974 "The occupations of the undergraduate sociology major." Teaching Sociology: 2:91–100.
Sherman, Lawrence W. et al.
1978 The Quality of Police Education. Washington, D.C.: National Advisory Commission on Higher Education for Police Officers.

Shin, Eui Hang
    1975 "Statistics requirements for undergraduate
          sociology majors in colleges and univer-
          sities in the United States." The American
          Sociologist 10:92–102.
Stevens, R. A. and C. Todd
    1977 "Employment patterns among sociology
          majors and their implications for career
          planning at Winona State University."
          Presented at the annual meetings of the
          Midwest Sociological Society.

Terry, Geraldine B.
    1979 "Our professional responsibilities toward
          job skills and job shopping for our B.S. and
          B.A. graduates." The Southern Sociologist
          10:3–6.

Vaughan, C. A. and J. Cech
    1977 "A career information packet for under-
          graduate sociology majors." Presented at
          the annual meetings of the Midwest
          Sociological Society.

SOCIOLOGY AND THE INTRODUCTORY TEXTBOOK*

ROBERT PERRUCCI

Purdue University

*Forty introductory sociology textbooks, ten from each of four time periods between 1958 and
1977, are examined with respect to their structure, content, and connection with research
contributions in sociology. The basic structure and content of textbooks have changed very
little over the twenty year period. Recent texts have fewer chapters than earlier books, with less
space being devoted to separate chapters on basic concepts, theories, and methods. "New" or
unconventional chapters rarely find their way into the textbooks. Analysis of the most
frequently cited sociologists, citations to research literature, and citations to ASA and SSSP
award books indicates that textbook knowledge is not very current, and does not accurately
convey to students the diversity that exists within the field.*

In colleges and universities across the
country, the first course in sociology en-
rolls upwards of 500,000 students.[1] For
the largest number of these students the
introductory course will be their only ex-
posure to the discipline while in college.
Some will take a second or third course in
social problems or marriage and the fam-
ily, which are the other undergraduate
sociology courses with the largest en-
rollments. Still others, probably the
smallest proportion, will major in sociol-
ogy as undergraduates, and will thereby
be exposed to greater breadth and depth in
the discipline.

The growth of the introductory course
has been accompanied by notable changes
in the number and kind of textbooks that
are produced. In the years 1973–1975
there were 25 new introductory sociology
textbooks published and 18 revisions of
previously published texts (Brown, 1976).
Adding to this the number of previously
published texts still available makes a
total of 66 books from which to choose.
The substantial number of texts suggests
that teachers of introductory courses have
a wide range of choice in selecting books
that vary significantly in approach sub-
stance, topics, etc. On the other hand,
many texts may contain only minor varia-
tions in content and focus instead on spe-
cial features such as color, marginal notes,
and "boxed" highlights to achieve a mar-
ket advantage over competing texts.

The growth in enrollments has placed
the introductory textbook within the do-
main of large corporate business. Market
considerations help to shape decisions
about who should write textbooks, what
they should contain, and how they should
be used by teachers and students. Recog-
nition of this fact has resulted in concern

---

[1] Estimates of the number of students who enroll
annually in the introductory sociology course are
found in Reichlin (1979) and McGee (1977).

about the consequences that might follow a textbook market dominated by large publishing houses and conglomerate corporations (McGee, 1977). Will small publishing houses be forced out of business, or absorbed, making it increasingly difficult to get anything but textbooks published? Will the decline of the traditional "authored" text, in favor of "author-assisted" and "managed" texts, result in greater distance between the textbook, the discipline, and students? (Geersten, 1977).

The introductory textbook should be of concern to sociologists for a number of more general reasons:

(1) The introductory textbook is the primary teaching device by which students first learn about sociology.

(2) Textbooks are presented, by authors and publishers, as representative of the field of study.

(3) The ideas contained in textbooks can shape a field of study through their impact upon teachers who use them and those students who may become professional sociologists.

In this paper, our concern with introductory sociology textbooks will focus on their content, their apparent purposes, and their connection with research contributions in sociology. With respect to *content,* special attention will be given to first chapters since they attempt to define the subject matter, identify the sociological approach, and interest the student in the book. The *purpose* of the textbook with which we will be concerned is its relative emphasis upon "professional reproduction" or "client-centered" interests (Liebert and Bayer, 1975). The relationship between the textbook as knowledge disseminator, and the field of sociology as knowledge producer, will be determined by examination of the books, articles, and sociologists cited in chapters of textbooks.

To achieve these goals, we examined 40 introductory sociology textbooks, ten from each of four periods: 1958–1962; 1963–1967; 1968–1972; and 1973–1977. The texts examined from the first two time periods are the same as those analyzed by Bain (1962) and Oromaner (1968). In each of the last two time periods we randomly

selected 10 texts from among all new texts advertised in the *American Sociological Review* for the time period in question. Table 1 contains a complete listing of the 40 textbooks examined in this paper.[2]

CHAPTER STRUCTURE AND CONTENT

The basic structure and content of the introductory text has changed very little in the twenty year period under consideration. Texts published in 1958–1962 were "bigger" books, averaging 23 chapters, than those after 1962, which averaged about 18 chapters. The modal pattern for recent texts is about sixteen chapters.

Chapters were classified according to a general scheme provided by Inkeles in order to estimate the degree of stability or change in the kind of chapters that appear in introductory texts. Table 2 contains a summary classification of text chapters in 40 books.

The general pattern is one of substantial stability in the type and distribution of chapters across a 20 year time period. Chapters dealing with basic "social institutions" are found in virtually every text, and they account for about one-fourth of all chapters. "Social process" subjects (social change, stratification, deviance, social movements) also are stable over time, accounting for about one-fifth of the chapters.

The number of chapters devoted to "sociological analysis" (basic concepts, theories, methods) have declined somewhat since the 1958–1962 period. During the first period it was not unusual to open a book with five or more nonsubstantive chapters dealing with basic concepts, theoretical perspectives, research methods, and the nature of science. More recent texts have combined these topics into one or two chapters devoted to sociological analysis.

---

[2] The books used by Bain (1962) and Oromaner (1968) include revised editions of previously published books. Books selected for the time periods 1968–1972 and 1973–1977 are only newly published first editions. Ideally, only first editions should be used if the intent is to see a text as a reflection of the state of the field at the time a book is published. However, our desire to maintain some continuity with earlier work led to the decision to use the books examined earlier by Bain and Oromaner.

TABLE 1 Introductory Textbooks Analyzed

1958–1962 (Bain, 1962)

Broom, L. and P. H. Selznick. Sociology: A Text with Adapted Readings. 2nd ed. Evanston, IL: Row, Peterson and Co., 1958.

Chinoy, E. An Introduction to Sociology. New York: Random House, 1961.

Cuber, J. F. Sociology: A Synopsis of Principles. 4th ed. New York: Appleton-Century-Crofts, 1959.

Green, A. W. Sociology: An Analysis of Life in Modern Society. 3rd ed. New York: McGraw-Hill, 1960.

Landis, P. H. Introductory Sociology. New York: Ronald Press, 1958.

Lundberg, G. A., C. C. Schrag, and O. N. Larsen. Sociology. Rev. ed. New York: Harper and Brothers, 1958.

Ogburn, W. F. and M. F. Nimkoff. Sociology. 3rd ed. Boston: Houghton Mifflin, 1958.

Sutherland, R. L., J. Woodward, and M. A. Maxwell. Introductory Sociology. 6th ed. Philadelphia: J. B. Lippincott, 1961.

Williams, R. M., Jr. American Society: A Sociological Interpretation. 2nd ed. New York: Knopf, 1960.

Young, K. and R. V. Mack. Sociology and Social Life. 2nd ed. New York: American Book Co., 1962.

1963–1967 (Oromaner, 1968)

Bensman, F. and B. Rosenberg. Mass, Class and Bureaucracy: The Evolution of Contemporary Society. Englewood Cliffs, NJ: Prentice-Hall, 1963.

Bertrand, A. L. Basic Sociology: An Introduction to Theory and Method. New York: Appleton-Century-Crofts, 1967.

Bierstedt, R. The Social Order: An Introduction to Sociology. 2nd ed. New York: McGraw-Hill, 1963.

Bredemeier, H. C. and R. M. Stephenson. The Analysis of Social Systems. New York: Holt, Rinehart and Winston, 1965.

Broom, L. and P. H. Selznick. Sociology: A Text with Adapted Readings. 3rd ed. New York: Harper and Row, 1963.

Gouldner, A. W. and H. P. Gouldner. Modern Sociology: An Introduction to the Study of Human Interaction. New York: Harcourt, Brace and World, 1963.

Lundberg, G. A., C. C. Schrag, and O. N. Larsen. Sociology. 3rd ed. New York: Harper and Row, 1963.

Smelser, N. J. (ed.) Sociology: An Introduction. New York: Wiley, 1967.

Vernon, G. M. Human Interaction: An Introduction to Sociology. New York: Ronald Press, 1965.

Wilson, E. K. Sociology: Rules, Roles and Relationships. Homewood, IL: Dorsey Press, 1966.

1968–1972

Allyn, C. Sociology: An Introduction. Englewood Cliffs, NJ: Prentice-Hall, 1972.

Caplow, T. Elementary Sociology. Englewood Cliffs, NJ: Prentice-Hall, 1971.

CRM. Society Today. Del Mar, CA: CRM Books, 1971.

DeFleur, M. O., W. V. D'Antonio, and L. B. DeFleur. Sociology: Man in Society. Glencoe, IL: Scott Foresman, 1970.

Dressler, D. Sociology: The Study of Human Interaction. New York: Knopf, 1969.

Himes, J. S. The Study of Sociology. Glencoe, IL: Scott Foresman, 1968.

Lowry, R. P. and R. P. Rankin. Sociology: The Science of Society. New York: Charles Scribner's Sons, 1969.

Lenski, G. Human Societies. New York: McGraw-Hill, 1970.

McKee, J. B. Introduction to Sociology. New York: Holt, Rinehart and Winston, 1969.

Popenoe, D. Sociology. New York: Appleton-Century-Crofts, 1971.

1973–1977

Baldridge, J. V. Sociology: A Critical Approach. New York: Wiley, 1975.

Denisoff, R. S. and R. Wahrmer. An Introduction to Sociology. New York: MacMillan, 1975.

Dushkin Publishers. The Study of Society. Guilford, CT: Dushkin Publishing Group, 1974.

Leslie, G. R., R. F. Larson, and B. L. Gorman. Order and Change: Introductory Sociology. New York: Oxford, 1973.

Light, D. and S. Keller. Sociology. New York: Knopf, 1975.

Robertson, I. Sociology. New York: Worth Publishers, 1977.

Shepard, J. M. Basic Sociology. New York: Harpers, 1974.

Stewart, E. W. and J. A. Glynn. Introduction to Sociology. New York: McGraw-Hill, 1973.

Smith, R. and F. Preston. Sociology: An Introduction. New York: St. Martins, 1977.

Warren, C. Sociology: Change and Continuity. Homewood, IL: Dorsey, 1977.

Topics dealing with "primary units" show some variability over time, but without a particular trend. This particular category simply may be too broad, in that it combines topics that are quite diverse and that reflect different levels of analysis. Primary units include chapters on primary groups and sex roles, as well as urban structure, complex organizations, and ethnic minorities.

What is notable in the 40 books is the infrequency with which new or unconventional chapters are introduced. The "other" category contains chapters on "uses of sociology," "social policy," "international problems," "social plan-

TABLE 2. Classification of Chapters in Introductory Textbooks, 1958–1977

	1958–1962	1963–1967	1968–1972	1973–1977
1. Sociological analysis (Soc. perspective; scientific method; nature of society)	17.1%	7.8%	11.1%	9.1%
2. Primary units (Groups; communities; organizations; personality; social relationships)	32.5%	49.2%	36.5%	41.9%
3. Basic institutions (Family; education; scientific; political; economic; religious; recreational-aesthetic)	25.8%	23.5%	22.2%	22.2%
4. Social processes (Stratification; conflict; socialization; social control-deviance; communication; change)	23.3%	18.4%	26.4%	22.0%
5. Other	1.2%	1.1%	3.7%	4.3%
	99.9%	100.0%	99.9%	99.9%

ning," and "social problems." Only 10 of 40 texts had one or more chapters that did not fit the conventional categories.

A much clearer picture of stability and change is obtained from the contents of opening chapters of textbooks. The early textbooks (1958–1962) were clear and unambiguous in their presentation of sociology as a science of society, closely aligned with the natural science model. Sociology is described as a value-free or value-neutral discipline whose practitioners can and should separate personal values from scientific work. Consider, for example, the following statements:

"Sociologists have no 'axe to grind' other than this interest in building up the most nearly perfect body of knowledge about human beings that they can . . . the personal predilections of sociologist A or of sociologist B are not a part of sociology" (Cuber, 1959:6).

"In regarding sociology as a natural science, we are studying human social behavior in the same objective spirit that a biologist studies a hive of bees, a colony of termites, or the organization and functioning of an organism" (Lundberg, et al., 1958:17).

"The sociologist may step out of his role as a research scientist and, taking the role of the citizen, educator, writer, or social engineer, use knowledge of the causes of crime to try to reduce the amount of crime" (Ogburn and Nimkoff, 1958:10).

In addition to the stress on sociology as an objective science in these early texts, there also is an explicit commitment to science as a value that contributes to societal progress. For example:

"Most people recognize the importance and promise of the advancement of science in general. It has released us from some age-old fears and insecurities" (Lundberg, et al., 1958:15).

"Since knowledge is enduring, and fresh discoveries are made from time to time, knowledge accumulates. The stockpile of sociology, of which this book is an indication, may even at the present time be viewed with pride. In the future we expect it to be much bigger. And a great accumulation of reliable and enduring knowledge in sociology will be inconceivably useful and effective in attaining our objectives of the good life" (Ogburn and Nimkoff, 1958:12–13).

"The scientist seeks scientific truth because he accepts, *on faith and faith alone*, that out of the quest for more accurate knowledge will eventually come some good for humankind" (Cuber, 1959:43).

Textbooks in the next period (1963–1967) maintained the emphasis on sociology as a value-free science, but omitted the "faith in science" or "science can save us" statements.

By 1968–1972, at least one-half of the textbooks discuss the existence of substantial controversy in the discipline on the possibility of a value-neutral sociology, and on ethical issues confronting sociologists and other scientists. There is a marked lack of "faith in science," and sociology is not presented as a consensus-based discipline. Such issues and questions engaged the attention of sociologists in the 1960s, and it is interesting to see the relative speed with which such matters find their way into introductory texts. It is especially in-

teresting in light of the slowness—to be discussed below—with which sociological knowledge finds its way into textbooks.

Textbooks in the most recent period (1973–1977) show some increase in discussion of issues of value and ethics in sociology. Six of the ten texts examined provide coverage of the topic. However, the most recently published books do not include such controversial topics, signaling, perhaps, a return to a blander and more consensus-oriented characterization of the field.

Another basis on which to contrast the opening chapters of our forty texts is whether different theoretical perspectives in sociology are presented. Specially noteworthy is whether students are exposed to theoretical frameworks, and whether the discipline is characterized as containing competing perspectives.

Textbooks in the earliest period (1958–1962) contain very little discussion of theoretical perspectives or frameworks, and only one book gave any indication that sociologists were in any disagreement on the matter of how the subject matter of sociology should be approached (this was on the epistemological differences between positivism and *Verstehen*).

The 1963–1967 texts continue the indifference to discussing specific theoretical perspectives, and only two texts provide any discussion of different orientations toward the subject matter by contrasting scientific, humanistic, and action-oriented sociology. This same pattern continues for the 1968–1972 textbooks. What is new in opening chapters in texts of this period is a discussion of the relevance of sociology in terms of application, and of the employment settings of sociologists. The question of what people with degrees in sociology actually *do* with their knowledge of human society is probably due to the same social forces in the 1960s that resulted in increased characterization of sociology as a diverse discipline. It probably is not the absence of employment opportunities that leads to discussion of employment settings for sociologists, but rather the involvement of a number of sociologists in policy-making and policy-implementing positions in organizations dedicated to social change.

In 1973–1977 texts, all but one of the books provide *some* discussion of diversity in sociology with regard to specific theoretical perspectives or orientations. Several texts give substantial space to discussion of functionalist, conflict, and interactionist perspectives; or to Marxism and phenomenology; or to sociology as an art and science. Nevertheless even when there is some discussion of different theoretical perspectives they rarely are carried forward and used in the remaining substantive chapters.

Finally, we compare the textbooks in each decade as to whether or not they will be interesting to students—intellectually exciting, challenging, or applicable to their lives. While this is a very subjective matter, the criterion is not unimportant, given that a student's experience in the introductory course may influence decisions to take other courses in sociology.

I approach this criterion indirectly by considering the kind of topics covered in the opening chapters. It appears that most textbooks in each time period devote an inordinate amount of space to matters of little interest to students and with little potential for engaging them with the subject matter. Opening chapters are, on the whole, extremely defensive about sociology as a science and preoccupied with distinguishing sociology from other disciplines. The patterns observed by Kurtz and Maiolo (1963) in their comparison of sociology texts with those in chemistry, economics, physics, political science, and psychology, are reproduced in the textbooks examined here. The arguments are so standard that they have the appearance of a litany. "Science is a method; sociology has a method; therefore sociology is a science." The setting of boundaries between disciplines invariably leads to a characterization of other fields that would leave our colleagues in journalism, economics, psychology, or political science believing us to be either mad or totally ignorant of what goes on in other disciplines.

It seems to me that students might be more attracted to sociology as a way of looking at the world if they were presented with a sense of how the various disciplines are interrelated and engaged in

the common endeavor of trying to answer certain persistent questions about human behavior and human society. It also seems that they would be more interested in the subject matter (and in the remainder of the text) if opening chapters were substantive, dealing with the societal problems and issues that sociologists deal with in their research and writing. Moreover, students might be more interested in sociology if the textbooks were more concerned with serving the personal and vocational needs of students rather than simply attempting to reproduce the knowledge of the discipline.

MOST FREQUENTLY CITED SOCIOLOGISTS

In 1962 Read Bain examined ten textbooks published between 1958 and 1962 to determine who were the most frequently cited sociologists. This study was repeated by Oromaner (1968) on ten textbooks published between 1963 and 1967. In both the Bain and Oromaner studies a name cited at least five times in four or more textbooks in the time period was classified as a "most frequently cited sociologist." We followed the same procedure for texts in 1968–1972 and 1973–1977.

The textbooks examined by Bain contained the names of 24 sociologists who had received five or more citations in four or more books. Oromaner found 23 such names for texts in the 1963–1967 period. Texts published in the 1968–1972 period contained 12 frequently named sociologists; and for 1973–1977 texts, the number was 11. Thus, textbooks in the two earliest time periods had about twice as many sociologists who are frequently cited as texts in the two most recent time periods.

The pattern of frequently cited sociologists across the four time periods is reported in Table 3. Only six names were frequently cited in all four time periods, and another three names were cited in the three earliest time periods. Seven sociologists were frequently cited in the two earliest periods, and one sociologist in the two most recent time periods.

There are several noteworthy aspects to the pattern of frequently cited sociologists. First, it appears that once a name is dropped from the frequently cited list in one time period, it does not reappear in a later time period. The impact of a sociolo-

TABLE 3. Names Cited at Least Five Times in Four or More Introductory Texts

Cited in all four time periods	Cited in three time periods (1958–62; 1963–67; 1968–72)	Cited in two time periods	Cited only in one time period	
K. Davis	C. H. Cooley	(1958–1962; 1963–1967)	(1958–1962)	
E. Durkheim	S. M. Lipset	R. Bendix	R. Bales	W. F. Ogburn
R. K. Merton	G. P. Murdock	E. W. Burgess	F. S. Chapin	W. G. Sumner
C. W. Mills		A. B. Hollingshead	P. K. Hatt	W. S. Thompson
T. Parsons		R. K. Linton	R. MacIver	
M. Weber		S. Stouffer	M. Mead	
		W. L. Warner		
		R. Williams	(1963–1967)	
			S. Freud	W. E. Moore
		(1968–1972 & 1973–1977)	G. Homans	T. Newcomb
		K. Marx	P. F. Lazarsfeld	E. Shils
				W. F. Whyte
			(1968–1972)	
			H. S. Becker	
			G. Lenski	
			(1973–1977)	
			H. Blumer	
			H. Gans	
			E. Goffman	
			G. H. Mead	

TABLE 4. Publication Date of Research Articles and Books Cited in Selected Textbook Chapters

	1968–1972 Texts Publication date of citation					1973–1977 Texts Publication date of citation				
Organizations chapter	Last 5 years	6–10 years	11–15 years	16 + years	Total	Last 5 years	6–10 years	11–15 years	16 + years	Total
Articles cited ($\bar{x}$ = 4.4)* ($\bar{x}$ = 2.5)**	3.2%	32.2%	16.1%	48.4%	(99.9%)	32.0%	28.0%	8.0%	32.0%	(100.0%)
Books cited ($\bar{x}$ = 7.1)* ($\bar{x}$ = 3.2)**	14.0%	18.0%	36.0%	32.0%	(100.0%)	6.2%	28.1%	21.9%	43.7%	(99.9%)
Stratification chapter										
Articles cited ($\bar{x}$ = 8.0)* ($\bar{x}$ = 7.9)**	14.0%	28.1%	20.3%	37.5%	(99.9%)	29.6%	26.8%	15.5%	28.2%	(100.1%)
Books cited ($\bar{x}$ = 13.2)* ($\bar{x}$ = 5.8)**	12.2%	18.9%	18.9%	50.0%	(100.0%)	7.7%	30.8%	21.2%	40.4%	(100.1%)

* Mean number of research articles and books cited in 1968–1972 textbooks.
** Mean number of research articles and books cited in 1973–1977 textbooks.

gist's work on the writers of introductory textbooks apparently follows an orderly pattern of influence across the time periods.

A second interesting aspect of Table 3 is the appearance of Marx as a frequently cited sociologist in 1968–1972 and again in the most recent time period. This appears to correspond with the appearance of discussion of diverse theoretical perspectives in textbooks, bringing in a conflict or Marxian mode of analysis.

The final interesting feature of the citation data is the influence, in the most recent time period, of sociologists representing specific social psychological perspectives (Blumer, Goffman, and Mead). They join the frequently cited sociologists of a structural or macro-analytical approach to provide a more balanced picture of "most important sociologists." Whether or not this social psychological emphasis persists must await an examination of textbooks in the 1980s.

CITATIONS TO RESEARCH LITERATURE IN SOCIOLOGY

Since one of the central objectives of the introductory text is to present sociology as a science of society, with a body of knowledge, we wish to know whether texts draw upon the knowledge produced by scholars in the discipline. We approach

the question first by examining the number of research articles and scholarly books cited in two selected chapters in textbooks published in the two most recent time periods.[3] Table 4 contains information on the number of articles and books cited in texts and their date of publication relative to the publication of the textbook.

Considering first of all the number of articles and books cited in the organizations chapter, we see that an average of 4.4 articles were cited in 1968–1972 texts, and 2.5 in 1973–1977 texts. The number of books cited also declines from 7.1 in 1968–1972 to 3.2 in 1973–1977. The number of citations of articles and books in the stratification chapter is larger, probably reflecting the longer tradition of research and writing in the area of stratification. Citations of articles remains about

[3] Research articles include traditional scholarly and professional periodicals and exclude references to popular social science periodicals like *Society* and *Psychology Today* and to news magazines like *Time* and *Newsweek*. Books include scholarly publications by university and commercial presses and exclude general textbooks in the areas of stratification and organizations. We did not include textbooks from the 1958–1962 and 1963–1967 periods because of the large number of second, third, and fourth editions included by Bain (1962) and Oromaner (1968). This would have required examination of all prior editions to determine when a particular citation appeared relative to the textbook's date of publication.

the same in both time periods, but the number of books cited drops from 13.2 to 5.8 in the 1973–1977 textbooks.

The articles and books cited are also classified according to when they were published relative to the textbook's publication. An article or book could have appeared in the last five years prior to the textbook's date of publication, 6–10 years, 11–15 years, or 16 or more years. These data provide some indication of how much of the recent scholarly literature finds its way into textbook chapters.

In the 1968–1972 period citations in the organizations chapter generally are quite "old." Almost half of the articles cited appear 16 or more years prior to the textbook's publication date, and about two-thirds are 11 or more years old. A similar pattern exists for the books cited, with over two-thirds being published 11 or more years prior to the text.

In the 1973–1977 texts there is an increase in the number of citations to recent articles in the chapter on organizations. Almost one-third of the cited articles were published in the five years prior to the textbook's publication, and 60% of the articles appeared ten or fewer years prior to the text. There is no comparable shift to citation of more recently published books in the chapter on organizations; in fact, there are more citations to older books than there were in the 1968–1972 texts.

Looking at citations of articles and books in the stratification chapter we see that a majority of the citations in the 1968–1972 texts are of "older" publications. About 57% of the articles cited are over 11 years old, and 69% of the books are of the same vintage.

When looking at the date of cited articles in the 1973–1977 texts, we find a pattern similar to that observed in chapters on organizations. There is a shift to citing more recently published articles, with over 55% of the citations being of work published in the last ten years (compared to 42% in the 1968–1972 texts). Citations of books show a slight tendency toward more citations of "older" works, similar to that found in the chapters on organizations.

In general, my review of these selected chapters in introductory textbooks indi-cates that they do not seem to synthesize the recent knowledge produced by scholars in the discipline. Citations of older work are substantial, indicating either that sociology has a large number of classic research articles and books, or that writers of introductory texts have not kept pace with the recent scholarly literature.

In order to provide some comparison with these data, I examined the citation patterns in an introductory textbook that was written by a collection of sociologists who can be considered "specialists" in the subject matter of each chapter. The "specialist" text was published in 1967, a time preceding the two time periods represented in Table 4, and preceding a decade of substantial growth in scholarly research and writing.

The number of citations of research articles in chapters on organizations in the "specialist" text is 7, and for books it is 25. This is larger than the number of citations reported in Table 4 for either time period of the nonspecialist texts. The dates of publication of cited articles and books show a definite tendency toward greater use of more recent literature. For articles, 85% of those cited were published in the last ten years (far greater than that reported in Table 4 for either time period). Citations of books also were of those published more recently, although the differences with the non-specialist text are not as great. Almost one-half of all cited books were published in the last ten years, compared to 32% and 34% of cited books in the two time periods of non-specialist books.

The stratification chapters in the specialist text contain 16 citations of articles and 27 of books. Both figures exceed that found for the non-specialist texts reported in Table 4, and the cited works are more than those reported in the non-specialist texts. About 44% of the articles cited in stratification chapters in the specialist text were published in the last five years (compared to 14% and 29% in the 1968–1972, and 1973–1977 non-specialist texts). For books cited, 37% were published in the last five years, compared to 12% and 8% in the non-specialist texts reported in Table 4.

Clearly, textbook chapters written by

"specialists" or "experts" draw upon a body of scholarly literature that has been published more recently than one finds in the non-specialist texts. It should come as no surprise to any sociologist that it is extremely difficult to remain current with the latest contributions in more than two or three sub-fields; and that textbooks written by single or several authors, or by professional writers with sociological advisors, or by teaching specialists rather than active scholars, will invariably fail to incorporate or synthesize the latest research literature.

CITATIONS TO ASA AND SSSP AWARD BOOKS

The final indicator of whether introductory textbooks reflect the scholarly activities of the discipline is the frequency with which books receiving the major awards of professional societies are cited. From 1957 through 1968, the American Sociological Association gave the MacIver Award for scholarly books making important contributions to sociology. From 1969 to the present time the ASA has given the Sorokin Award to recognize outstanding contributions. Since 1964 the Society for the Study of Social Problems has provided an annual C. Wright Mills Award for the book that best exemplifies scholarship in social science in the tradition of C. Wright Mills.

Table 5 contains the frequency and percentage of award books that are cited in introductory textbooks in the four time periods. An award book was considered eligible to be cited if it had received the award at least five years prior to the publication of the introductory textbook. This was done in order to give textbook writers sufficient time to become aware of the books selected for the highest honors of the discipline.

Looking first at the ASA MacIver and Sorokin Award books, we observe a clear decline in the percentage of eligible award books that are cited in texts. In 1958–1962, 43% of the 21 possible citations to award books were found in the ten texts of the period. This figure declines to 24% of the 169 possible citations that could have appeared in the 1973–1977 period. The range of possible citations provided in Table 5 gives an indication of the "worst" and "best" citation records found in particular textbooks.

Figures for the SSSP Award are provided only for the most recent time periods, because this award was not given until 1964. The available data indicate that a smaller percentage of eligible C. Wright Mills Award books are cited than those receiving the MacIver and Sorokin Awards. This seems significant because the SSSP Award is for books on social problems with a critical orientation, while the ASA awards can be viewed as going to books oriented to "science-building."

The substantial increase in the number of books eligible for citation in recent time periods could be seen as the main reason for the sizable decline in the percentage of possible citations found in textbooks. In order to check on this possibility, textbooks in the two most recent time periods were examined for citations to books receiving ASA awards in the ten year period before publication of the textbook. In this way we can see if the recent textbooks have a better citation record when the number of possible citations is reduced. The data do not support this speculation. Textbooks published in 1968–1972 cited only 20% of the 50 citations that were possible. In 1973–1977,

TABLE 5. Citations of Books Receiving ASA MacIver or Sorokin Awards and SSSP C. W. Mills Award

	1958–1962	1963–1967	1968–1972	1973–1977
ASA Awards				
Percent cited in 10 texts in each period:	43%(9/21)	35%(16/46)	23%(26/111)	24%(41/169)
Range:	0/2–3/4	0/11–3/4	1/11–5/13	1/14–6/17
SSSP Award				
Percent cited in 10 texts in each period:	—	—	16%(6/37)	15%(15/97)
Range:	—	—	0/6–2/7	0/14–4/6

22% of the possible citations were actually cited in the textbooks of the period. In each case, the percentage is not very different from the citation percentages reported in Table 5 that are based on all possible eligible citations to both "new" and "old" award books. In short, citation of award books is not related to how recently that award book was published.

DISCUSSION

The examination of 40 introductory sociology textbooks covering a 20 year period indicates that textbooks are caught between two objectives, falling short of both. One objective is to provide students with exposure to what are thought to be the foundations, basic principles, and survey of the field. This goal has been described by Leibert and Bayer (1975) as "professional reproduction," and by Kahout (1977) as the "vocational model." It assumes, implicitly, that students are being prepared for further course work in sociology and for a career in the field.

The second goal is more student-centered; it attempts to link knowledge of sociology to the everyday lives of students and to prepare them for their futures as workers, citizens, and parents. Student-centered goals frequently are mentioned in the first chapter of textbooks, or in a special preface to students about "why this book was written, and how it should be used." Unfortunately, these purposes rarely find their way into the remaining chapters.

Our review indicates that virtually all of the textbooks examined are dominated by the goal of professional reproduction. Thus, my summary remarks are designed to assess whether they adequately represent the field of sociology.

(1) By and large, introductory textbooks do not accurately convey to students the tremendous diversity of views that exists in the field regarding theory, approaches to the study of social phenomena, and the purposes of sociology. The image of the field contained in textbooks suggests that sociologists share a common view of their discipline, and that there is, therefore, theoretical coher-

ence and consensus on the questions to be examined, and how to go about dealing with them. We apparently all share the goal of creating a body of scientific knowledge about society.

(2) Given the overriding objective of "professional reproduction," our examination of selected chapters indicates that the information presented to students as sociological knowledge is based upon citations to scholarly literature that is somewhat dated.

Our analysis indicates only that textbook knowledge is not very current. Whether or not that knowledge is accurate is another matter. Recent examination of textbooks in marriage and the family by Spanier and Stump (1978) and industrial sociology by Hadley and Taveggia (1977) should make us very wary about how research findings are reported in textbooks. Spanier and Stump's analysis of 18 texts found that they drew heavily on secondary sources, reported research findings without citations, selectively reported findings to produce misleading conclusions, confounded definitions of variables used in the original research, and often drew change-based conclusions from cross-sectional data. Hedley and Taveggia looked at 14 textbooks to determine the treatment given to a specific empirical generalization that was reported in 31 research articles. They found that approximately one-third of the studies were described in the textbooks as having supported a particular conclusion that was in no way related to the original research data. If one assumes that introductory textbooks are subject to the same kinds of errors and inaccuracies, as well as being dated, then the representation of the field of sociology in textbooks should be a matter of concern.

In conclusion, I hope that sociologists who write introductory textbooks in the next decade (if there are any) will attempt to break away from the image of the textbook created by market research, which makes for increasing standardization of content. Some textbooks should be addressed to the overwhelming majority of students who never will become "majors," and who never may take a sec-

ond course despite our efforts. The first course for such students should be more substantive than at present, more akin to a social problems course. And it should be more of an introduction to the sociological way of looking at the world[4] than a survey of the field.

REFERENCES

Bain, R.
1962 "The most important sociologists?" American Sociological Review 27:746–748.
Brown, R. E.
1976 "Introductory texts: A report on current models." Contemporary Sociology 5:123–130.
Geertsen, R.
1977 "The textbook: an ACIDS test." Teaching Sociology 5:101–120.
Hedley, R. C. and T. C. Taveggia
1977 "Textbook sociology: some cautionary remarks." The American Sociologist 12:108–116.

---

[4] An excellent example of such a text is Schneider (1975). Although this book is demanding of the student, it also is very challenging, and could serve as a model of what an introductory text for nonmajors should look like.

Inkeles, A.
1964 What is Sociology? Englewood Cliffs, NJ: Prentice-Hall.
Kahout, F. J.
1977 "The first course in sociology." Teaching Sociology 5:37–48.
Kurtz, R. A. and J. R. Maiolo
1968 "Surgery for sociology: the need for introductory text opening chapterectomy." The American Sociologist 3:39–41.
Liebert, R. J. and A. E. Bayer
1975 "Goals on teaching undergraduates: professional reproduction or client-centeredness." The American Sociologist 10:195–205.
McGee, R.
1977 "The college market in commercial publishing: the case of the convertible." The American Sociologist 12:102–107.
Oromaner, M. J.
1968 "The most cited sociologists: an analysis of introductory text citations." The American Sociologist 3:124–126.
Reichlin, S.
1979 "The introductory sociology text." Unpublished manuscript, Department of Sociology, University of Pittsburgh.
Schneider, Louis
1975 The Sociological Way of Looking at the World. New York: McGraw-Hill.
Spanier, G. B. and C. S. Stump
1978 "The use of research in applied marriage and family textbooks." Contemporary Sociology 7:553–562.

IMPLICATIONS OF COMPETENCY-BASED EDUCATION FOR UNDERGRADUATE SOCIOLOGY*

WILLIAM J. WOOLF, JR. and JAMES M. BISHOP
Alverno College

Implications of college-wide competency-based education (CBE) approaches for undergraduate sociology programs are discussed, using an established program for illustration. Five basic questions for sociologists considering involvement in CBE are addressed: (1) Are courses to be built upon content or competency? (2) Is it possible to achieve the ideal of integrating content and competency in evaluating student performance without relegating content to a secondary role? (3) Is it possible to alter one's conception of teaching from dispensing knowledge to facilitating learning? (4) Is it possible to alter one's conception of professional autonomy to meet CBE demands? (5) Is the education at CBE institutions comparable to that offered at traditional institutions, both in terms of learning and career opportunities?

INTRODUCTION

This paper examines some implications of college-wide competency-based education (CBE) approaches for undergraduate sociology programs and curricula, using an existing program for illustration. A major difficulty in delineating CBE's impact on academic programs is that the approach is not yet marked by a set of unifying conceptual or organizational principles, nor does an adequate definition or theoretical framework exist to guide generalization (Grant, 1979:11). For these reasons, the present paper is confined to issues arising from elements that appear to be common to CBE approaches in general.[1]

While the CBE movement currently is not widespread among American colleges, interest in it—both positive and negative—is sufficiently great to suggest the likelihood that it will become the context for at least some sociology programs,

particularly those in smaller, non-elite colleges (cf. Grant et al., 1979). In this sense, CBE represents the foundation of one possible scenario for sociology curricula in the near future. Our purpose is neither to promote nor decry this likelihood, but rather to outline some of the constraints and possibilities, as well as some of the advantages and disadvantages, of CBE as a context both for developing and maintaining viable sociology curricula and programs. Our intent is descriptive rather than prescriptive. We hope to provide a sounder point of departure than now exists for those sociologists and others who may become involved with CBE.

Specifically, the implications of a CBE context are discussed for three general areas of interest: (1) curricular goals, evaluation, and methods; (2) faculty roles and development; and (3) educational outcomes.[2] We will draw tentative general-

---

[1] Gamson (1979:225) describes the common features of CBE programs as follows: "(1) the specification of educational outcomes reflecting successful functioning in life roles, (2) the view that instructional time is independent of the achievement of these outcomes, and (3) the certification of the achievement of the outcomes in a reasonably objective and verifiable way."

[2] The editors and some reviewers suggested that we discuss what implications derive from faculty/administration relationships, especially with respect to the institutionalization of CBE programs. While this is an important issue, we do not feel there is adequate basis for even tentative generalization in the literature or in our own experience. Some body, for example, must coordinate curricula and instruction for competencies so that the full range is covered on a college-wide basis. However, this may or may not be the administration itself. Our general impression is that whether CBE is instituted from above or arrived at through full democratic participation of faculty, the issues we have identified will remain.

izations from one CBE program that has passed beyond the initial growth stages of development. While not every issue we identify here appears in that program, it has served as a beginning point for our discussion. Thus, as a means of familiarizing the reader with the CBE context, its general features will be illustrated by reference to the curricular program at Alverno College, where CBE was initiated on a campus-wide basis in 1973.

Alverno College is a small liberal arts college for women in Milwaukee. It has a national reputation as a model CBE institution, its innovative program having thrust it rapidly into the forefront of the competency-based movement (Grant, et al., 1979; Edgerton, 1977; Chickering, et al., 1977; Levine, 1978, Chapter 9).

Education at Alverno centers on the concept of teaching for "behavioral outcomes" (demonstrated abilities) rather than on particular substantive content and finite time periods for learning it. The program identifies eight major outcomes, called "competences," for which students must exhibit specified levels of performance in order to graduate. Accordingly, the Alverno faculty see each competence as "generic, rather than simply specific to a task or situation . . . While they are learned and demonstrated in specific settings, these generic abilities can be transferred to a variety of situations" (Alverno College Faculty, 1979:7). The eight generic competences are:

1. Effective communications ability
2. Analytical capability
3. Problem solving ability
4. Valuing in a decision making context
5. Effective social interaction
6. Effectiveness in individual/environment relationships
7. Responsible involvement in the contemporary world
8. Aesthetic responsiveness (Alverno College Faculty, 1979:5).

Students are required to complete four "levels" of achievement in each competence area and eight advanced levels in selected areas in which they specialize (Alverno College Faculty, 1976:8). Only selected competences and sequential levels are offered in any given course,

these being selected by individual instructors. The range of competence and level offerings in courses are reviewed regularly by committees of faculty (Competence Divisions) responsible for particular competences (Alverno College Faculty, 1976:7).

Alverno's emphasis on both knowledge and ability means that the concept of learning becomes far broader than the definition traditionally associated with it at the college level. Through the procedures collectively labeled "assessment," faculty attempt to observe and evaluate the individual student in this larger context of learning. Assessment is not viewed as equivalent to traditional testing—i.e., objective tests and essay exams—but as both evaluating and helping students to develop their abilities rather than merely testing what they know. At Alverno, assessment focuses on the generic aspect of the competences, a developmental learning pattern for the students, and a "holistic" or cumulative sequence in helping students develop basic abilities (Alverno College Faculty, 1979:7–12). Related to this is the policy of not assigning grades to describe or rate student performance. Instead, written statements and narrative transcripts are used to specify student development. Alverno's concept of assessing student performance is not unique, but rather a feature of most CBE institutions. In the following discussion, wherever illustrations from Alverno's program could be used to clarify our argument, this has been done. It should be kept in mind, however, that our argument is general and not intended to be descriptive of the operation of CBE in any single institution, including Alverno.[3]

IMPLICATIONS OF CBE FOR CURRICULAR ORGANIZATION

Curricular Goals

Perhaps the most controversial issue generated by CBE for curriculum organization involves whether courses are to

___

[3] For more detailed information on CBE at Alverno College, see Alverno College Faculty (1976; 1979). For a critical view of the development of CBE at Alverno College, see Ewens (1979b).

be built upon substantive content or upon the competences being assessed. For example, a sociology course might be developed using such "generic" competencies as problem solving ability or understanding of individual/environmental relationships as a foundation. In the case of the latter, some substantive content is at least implied, a fact that can operate to alter—either by limiting or expanding—what otherwise would be considered course content. Moreover, where teaching toward each competence is divided into "levels," the instructor's approach to a given substantive area will in many ways be predetermined by the level being offered in the course. At Alverno, for example, the first two levels of "Analytical capability" require that students be able to identify implicit and explicit elements of a problem or idea and to infer implicit elements as well (Alverno College Faculty, 1976:15–16). An instructor offering only these first two levels of the competence would be overstepping formal instructional limits for instance, by asking students to comprehend the unity and integration of a work like Durkheim's *Suicide,* since this is a "higher level" competency. One solution is to offer a wide range of levels for a given competence in the same course, but this too has its problems unless all students enrolled are at a stage where they can legitimately be asked to perform at all those levels.

If course prerequisites generally are non-substantive, requiring prior mastery of specified levels of given competencies rather than prior coursework, students in advanced courses will not necessarily have what in traditional programs would be considered adequate preparation in basic sociological concepts or modes of thinking. For example, students *may* be unusually well prepared, compared with those in more traditional programs, to engage in analytic thought with respect to course content, but may need more than ordinary background preparation in elementary sociological principles and concepts. The problem for the instructor becomes one of balancing the need for sophisticated teaching and learning in the competency area against deficiencies in students' knowledge of the substantive

content to which the competency is to be applied. A greater readiness to engage in analysis, for instance, is a potential advantage, but the lack of a sense of sequence and substantive integration will be bothersome for all concerned.

In most CBE approaches, assessment for basic competencies appears to take precedence over substantive evaluations. In part this is due to a self-conscious effort among CBE curriculum designers to avoid traditional lecture and "testing" situations. For the present, however, assessment of even "generic" competencies requires *some* content as a requisite of demonstrating the ability in question (Ewens, 1979a). At Alverno, this issue has been addressed formally by officially stating a commitment to both content and competence in instruction (Alverno College Faculty, 1976:4–5). Undoubtedly, the ingenious instructor will see to it that the content is relevant to substantive course objectives. But the more fundamental issue is whether and how the infusion of substantive content can be assured in those programs where only the assessment of "non-substantive" abilities is institutionalized.

One possible method for providing such assurance is to have faculty establish competencies (and levels, if these are involved) on the basis of doing the least "damage" to course content. This is not a fool-proof resolution, because the competencies or levels chosen may allow for restricting content (e.g., in a course on theory) to some favored but limited topic (e.g., where the chosen competency concerns understanding of the environment at an advanced level). Alternatively, competencies could be established as a matter of departmental policy for specific courses, but this constrains instructors in their ability to deal with course content in ways they feel are most appropriate as sociologists.

These problems are not dissimilar to those encountered in attempting to hold faculty in traditional colleges accountable for what they teach and for the results of that teaching. CBE programs do not necessarily ensure accountability any more effectively than other approaches. However, the accountability issue is more

salient in the CBE context because both the constraints and the opportunities for faculty in developing course materials are relatively visible to all involved. Thus it is common in CBE programs for faculty to make available to students and colleagues alike detailed syllabi that include course goals and objectives, assessment procedures, and specific criteria for successful performance.

Evaluating Student Performance

The central issue in evaluating student performance in CBE institutions involves the difference between the ideal of integrating content and competency in assessment and the reality that tends to make content secondary. This issue is important because CBE easily leads to forming a dichotomy between competence and content. Particularly when competencies are held to be generic, the integration of content with competency in constructing assessment instruments is not guaranteed by formal statements of instructional goals. Only committed and creative instructors seem able to provide this certainty.[4]

In evaluating student performance, a competency potentially limits content by restricting the type of knowledge appropriate to development of the student's sociological abilities. A competence such as Alverno's "Valuing in a decision-making context," for example, may work reasonably well in an assessment involving exchange theory, but presents difficulties for the instructor interested in using the assessment for evaluating knowledge of Mead's theory of socialization. Of course, problems like this can be alleviated by offering a number of competencies in a given course, or by administering more traditional testing alongside competency assessments. On the other hand, competencies may provide useful vehicles for assessing particular content. For instance, a compe-

tence such as "Effective social interaction" is ideal for issue-oriented content, as in a Social Problems course, since it promotes discussion and critical analysis. Thus, building assessment instruments around content can be a good means for assessing a variety of competencies.

A related theme in the evaluation of student performance, one not ordinarily found in traditional departments, involves the question of who develops the assessment instruments. Three groups potentially can develop assessments for courses in CBE institutions: the individual instructor, the department, and/or an institutional group responsible for a particular competency. Assessment instruments developed outside the course context obviously have the potential of restricting the type of content covered. A generic instrument intended for use across a variety of departments might be effective for a course in complex organizations, for example, but not for a course on theory or social psychology.

One might imagine that close integration of content and competency would follow when individual instructors develop the assessment instruments for particular competencies. A crucial constraint on individual faculty, however, is the amount of time needed to develop such comprehensive instruments. In addition, certain content that an instructor feels is important may not be the most efficient for assessing a particular competency. Therefore, additional assessments concerning only content have to be developed. Again, given such constraints, the ideal of integration hardly can be guaranteed.

Student outcomes are the essence of CBE programs, and evaluation of student performance accordingly requires that assessments be as unambiguous as possible in terms of their intent and the criteria for judgment. CBE assumes that outcomes are objective and verifiable, making criteria for successful performance possible. These criteria, ordinarily supplied to students prior to assessment, provide ideally clear statements of faculty expectations as to what is considered competent performance in an assessment. This has the secondary effect of offering

---

[4] This statement must be qualified for those programs where CBE is oriented toward abilities that clearly are technical in nature (e.g., specific surgical skills in a medical program), rather than toward generic competencies. In such cases, content and competency are in effect one and the same.

the potential for greater instructor accountability as well. One issue is whether the criteria serve more as a checklist allowing students to structure minimal performances, thus stifling creativity, or merely as a formal guide for judging student achievement. This is especially relevant since CBE operates under the rubric of helping students develop their abilities. The problem for the instructor is to formulate specific behavioral objectives without artificially limiting student capabilities for maximum performance.

In addition to providing content through lectures, readings, and other course input, CBE approaches also provide opportunities for practice in the assessment for competency. This provision of information on the method of assessment, as well as on the substantive basis for assessment, underscores the practical emphasis inherent in the CBE approach. An instructional problem arising from this practice is how to determine whether students who are reassessed in the same competency until they succeed truly have mastered the competency or simply have become "test-wise." One solution is to offer different "practice" versions for the same competency, although this may raise questions as to the identity between different versions. Here is one example, in summary form, of an assessment used at Alverno to assess at the fourth level of "Understanding individual/environment relationships": "Having read and discussed Edward Hall's *The Hidden Dimension*, for instance, she is asked to design a cafeteria for the UN building, taking into account the wide differences Hall outlines in cultural space/intimacy patterns" (Alverno College Faculty, 1979:9).

Methods of Teaching

A central feature of CBE as opposed to more traditional approaches is that the conception of teaching changes from one of dispensing knowledge to one of facilitating the learning abilities of students. An important implication of this is that the lecture is no longer the optimal format for instruction. Being a learning facilitator means spending more time on student-centered learning experiences.

This creates pressure on faculty by limiting the amount of time for group instruction. One result of this approach to teaching is increased use of technical devices (e.g., video and audio tapes, films, etc.) in the classroom. Increasing the reliance on technical devices means more and various kinds of student-centered activities can occur than otherwise would be possible. At Alverno, for example, a student assigned to make an oral presentation often videotapes the presentation outside the classroom setting. When the instructor has time, the videotape is then viewed and assessed. Of course, one issue that also is raised by this aspect of the approach is its effect on departmental budgets.

Teaching in a college-wide CBE program always means contact with others outside one's own discipline. This cross-disciplinary contact has various implications for instruction. One is that faculty will have numerous contacts with individuals in the institution involved in the development of a competency offered in their course. At Alverno this point is particularly important in that competences are defined and developed apart from departments by faculty from different disciplines (Alverno College Faculty, 1976:7). New ideas and ways of teaching competences are generated by diverse sources throughout the college. Many times these approaches are appropriate for inclusion in sociology courses, but not always. Another aspect of CBE is that when emphasis is put on teaching for competency rather than content, instruction logically becomes an interdisciplinary matter. For the sociologist, then, both course development and team teaching with non-sociologists become more common. However, as teaching approaches vary to some extent among disciplines, interdisciplinary cooperation does not necessarily lead to development of more effective instructional techniques.

IMPLICATIONS FOR FACULTY ROLES AND DEVELOPMENT

The major consideration arising from the CBE approach for the role of faculty and for professional development is that conceptions of professional autonomy de-

rived from traditional academic contexts must be revised. In the CBE context, this is less an issue for debate than an inherent part of the approach's logic to which faculty must adjust.

Erosions of this traditional conception of faculty autonomy have numerous sources. One of these is that the amount of time and effort spent on developing, conceptualizing, and operationalizing assessments for competencies carries a weight that is at least equal to, if not greater than, time spent on disciplinary matters. For instance, at Alverno, faculty commonly belong not only to content-based Divisions (e.g., Behavioral Science), but also to one or more Competence Divisions (e.g., for "Effective social interaction"), charged with developing and maintaining criteria for specific competences and their assessments. Because it is so extensive, the time spent in competence divisions is roughly analogous to time spent in more traditional institutions on personal research projects in the discipline.

The internal, program-oriented focus typical of CBE institutions is one reason why research and publication tend to be considered personal matters, which become institutionally relevant only to the extent that they interfere with CBE instructional goals. The freedom of individual faculty to engage in professionally relevant research activity (defined in traditional terms) is thus not directly, but indirectly, curtailed due to such practical considerations as lack of time, relevant resources, and institutional encouragement. Of course, there are many faculty for whom work in the competency areas is a satisfactory alternative to the "publish or perish" motives operating in traditional settings.

Another factor associated with CBE that forces alteration of conventional faculty orientations toward autonomy is that the educational process is decidedly student-oriented rather than focused on substantive content or faculty expertise. As mentioned, faculty roles revolve around being facilitators of learning rather than dispensers of knowledge. While this builds acceptance of personal responsibility into the learning process for students,

it leaves faculty without clear-cut roles that can be associated with their professional identities in the field. To the extent that autonomy can be defined, it must be done largely without reference to specialized training or disciplinary identity. Faculty are held accountable for the "products" of their efforts as instructors, but mostly without regard for their substantive expertise.

An additional aspect of the student-centered nature of CBE programs is that faculty ranks, while they may exist even in traditional form, are relatively unimportant to careers within the organization as well as to considerations of power or expertise. Whether correctly or not, traditional ranking systems imply gradients of professional service and successful application of substantive skills. In CBE systems where facilitation of learning in non-substantive areas is the focal task, ranks and titles are not only less important, but are rarely even used or given reference. For one thing, the emphasis on facilitation rather than on dispensing knowledge transforms faculty into "trainers" of student "trainees." Moreover, the use of ranks or titles implies an importance for subject matter that is simply too great for CBE ideology. A result of this and related factors is that faculty must not only alter their conceptions of autonomy but also the reference point for their professional identities and careers. A Marxist or functionalist sociologist may remain so, but the matter is largely irrelevant in this context.

Interestingly, one alternative basis of identity may be found in involvement with the competencies themselves. At Alverno, for example, many faculty become as committed to and involved with the competence they represent by their membership in Competence Divisions as they do in their own disciplines, if not more so. This is facilitated not only by the sheer amount of time one must spend involved in working with the competence, but also by the fact that these Divisions are made up of members of many different disciplines cross-cutting the sciences, humanities, professional schools, and the social sciences. Since the only common denominator here is often the work in-

volved on a specific competence, colleagueship and sense of identity form around the competence itself, as does a sense of proprietary "right" over that competence and its fate in the overall program.

In CBE professional development acquires a new meaning. Reading and keeping up with one's discipline enjoy little officially sanctioned time. More than this, one may well ask what relevance these activities might actually have in a system where it is the generic rather than the ephemeral substantive that takes priority. In the absence of their conventional supports, autonomy and professional identity are redefined parochially in terms of creative work on competencies and their assessment, and on being an official monitor of their fates. So too, one's professional development must then be measured in these terms as well.

Advising is a role with heightened importance in CBE programs. It is through advising that students come to comprehend the complexities of the CBE system, and that faculty monitor their students' progress. Advising is a time when the faculty can continue their roles as learning facilitators outside the classroom. Ideally, one of the advisor's roles is to stress that students are building on their own knowledge base, and this makes a qualitative difference in student/faculty relationships. While students do not view faculty as peers, they are less mystified, more open, and willing to question faculty behavior than is the case in traditional institutions. Informal relations are likely to be stressed over formal, traditional relations. For instance, at Alverno, much formal and informal advising occurs in the cafeteria and other informal settings on a daily basis. While part of this type of association between students and faculty is undoubtedly due to Alverno's small size, the general CBE emphasis on "trainer"/"trainee" relationships helps to promote such an atmosphere.

For a CBE institution, recruitment of new faculty poses special issues due to the crucial role new members are expected to play in maintaining the system. Ideally, recruitment focuses on faculty who are strong in content, but also willing to commit themselves to teaching, students,

CBE, and the institution. In a tight job market, such as the one facing sociology at the present time, this means that research-oriented Ph.D. sociologists are likely to apply for teaching positions at student-oriented CBE institutions. Those who do must be willing to adjust their definitions both of the role of sociology and their own roles as academic sociologists to fit CBE institutional requirements.

Overall, the role faculty performs is critical to the survival of the CBE institution. A real problem, however, can be a high rate of faculty "burnout." As indicated, faculty work load is heavy. Time management is not a solution, however, since institutional expectations on the faculty mean long, although often personally rewarding, hours. Even though the total number of hours worked per week may not differ substantially between CBE and traditional institutions, the type of work is qualitatively different. Attempting to keep up with one's discipline or to do research means balancing student and institutional demands, not to mention personal obligation, against the desire to maintain and improve one's professional expertise. In general, there appears to be no adequate means of dealing with burnout, since the conditions that promote it are essential parts of the CBE approach.

IMPLICATIONS OF CBE FOR EDUCATIONAL OUTCOMES

Students enrolled in CBE institutions will ask whether their education will be comparable to that of their peers in traditional programs. At present, there is no clear evidence to provide an answer to this question. CBE offers several advantages to the student in terms both of career opportunities and gaining a grasp of the educational process itself. One of these is that the CBE approach requires spelling out instructional objectives and expectations for students very clearly. This is a result of the orientation toward measurable learning objectives. In addition, the sociology major, for example, can readily see the connections between various required courses and the broader competency requirements. The rationale for stressing one set of competencies over

others in a major program also should be spelled out clearly. Alverno's sociology program, for instance, currently focuses on "Analytic capability" rather than on other competences, as this is thought to provide the most sound conceptual base for examining values, one's relation to the larger society, and contemporary issues. In addition, the "Effective problem solving" competence is used as a groundwork for instruction in sociological methods.

The CBE approach also assists students by spelling out specific abilities they can draw upon in seeking employment or building a career base. With its emphasis on competencies rather than on relatively abstract course materials that may be difficult to describe or justify to prospective employers, CBE attempts to feed its "products" into the existing technological system. The terminology used for describing competencies is already familiar to employers within the business sector. While there are obvious value questions here, the immediate result for the student is an educational background that is in many ways more marketable than that possessed by competitors from traditional programs. This aspect of CBE may have special appeal for its present clientele, which tends to consist of first-generation, upwardly mobile students seeking a "place" in the established system (Grant, 1979).

There are also potential disadvantages for students associated with the CBE approach. A minor one is that CBE's association with the behavioral sciences makes the use of jargon terms common among participants. This serves to mystify outsiders and promote misunderstanding within the institutions, particularly among new students. For example, at Alverno students and faculty alike employ many jargon terms from behavioral psychology (e.g., "behavioral outcomes") to describe internal features of the program, and use numerous acronyms such as "CLU's" (competence-level-units), "8–5's" (Aesthetic responsiveness, level 5 of assessment), etc.

A more crucial question arises from the tendency of CBE programs to use narrative transcripts and other qualitative procedures for reporting student progress (see Gamson, 1979). While these provide a qualitatively better description than conventional letter grades for students as whole persons, they also can be misused or misinterpreted. It is usually not appreciated by the public as consumers of these transcripts that the information they provide depends heavily on the quality of the data base used to generate it, as well as on the amount of effort that faculty put into constructing it. A danger is that the large masses of data generated over a four-year college career can become so overwhelming to the transcript's authors that only superficial aspects of student performance are recorded. The quality of information a transcript conveys is obviously a function of such matters as student-faculty ratios, faculty workload, and the ability of instructors to describe in writing the learning process and outcomes for individual students. On the positive side, the transcripts function as an additional data source for employers or graduate school admissions officers, who can be persuaded that mastery of basic competencies is as relevant to their interests as successful completion of substantive course work.

A final implication for educational outcomes stems from the fact that CBE stresses the development of students' basic capabilities instead of only testing for substantive knowledge. That is, the fear of failure is radically decreased in comparison with traditional programs, and may even be eliminated altogether. CBE students generally progress at their own rate and have multiple assessment and reassessment. An obvious question this raises is whether some students are really learning by continually taking reassessments—i.e., without coming to terms with their own capabilities and limitations. Another possibility arising out of this issue, of course, is that weaker students may function to support the institution's interests in enrollment, while not recognizing that their own interests are not fully being served.

IMPLICATIONS FOR SOCIOLOGY

Various benefits as well as risks are associated with CBE approaches for undergraduate education in sociology. Any individual or department faced with the

choice of becoming involved in a CBE program should expect to deal with at least the following questions: (1) Are courses to be built upon content or competence? (2) Is it possible to achieve the ideal of integrating content and competence in evaluating student performance without relegating content to a secondary role? (3) Is it possible to alter one's conception of teaching from dispensing knowledge to facilitating learning? (4) Is it possible to alter one's conception of professional autonomy to meet CBE demands? (5) Is the education at a CBE institution comparable to that offered at traditional institutions, both in terms of learning and career opportunities?

For sociologists considering involvement with the CBE approach a new challenge is offered to their capacity for exercising "sociological imagination" (Mills, 1959). That is, CBE presents a context that may not be amenable to normal sociological skepticism. The private concerns of the sociologist *qua* sociologist are met with a set of issues generated by the characteristics of CBE that require not only understanding, but personal resolution before professionally meaningful action is possible. Some of the most salient of these issues are described in this paper. Others must be discovered by those who may choose one or another CBE program as a setting for their work.

In general, the emphasis in the CBE curriculum on "outcomes" and "behavioral objectives" can be identified with the increased technical rationalization of postindustrial capitalism (e.g., Habermas, 1970; Marcuse, 1964). Since its role in society is one of training people, in particular "new students," to take their "place" in society in positions they otherwise would not hold, CBE does potentially serve the existing technological system (Grant, 1979). Under these circumstances one challenge for sociologists working within CBE programs is to create the means for maintaining the spirit of sociological inquiry and skepticism within that context.

For some sociologists this challenge will prove to be too inconsistent with their sociological vision. For others, the challenge will be met with full professional zeal. It is our hope that the present paper offers a sound basis for guidance in either camp.

REFERENCES

Alverno College Faculty.
    1976   Liberal Learning at Alverno College. Milwaukee: Alverno Publications.
    1979   Assessment at Alverno. Milwaukee: Alverno Productions.
Chickering, Arthur W., David Halliburton, William H. Berquist, and Jack Lindquist.
    1977   Developing the College Curriculum: A Handbook for Faculty and Administrators. Washington, D.C.: Council for the Advancement of Small Colleges.
Edgerton, Russell.
    1977   "Education, work, and FIPSE." Pp. 110–124 in D. W. Vermilye, editor, Relating Work and Education. San Francisco: Jossey-Bass.
Ewens, Thomas.
    1979a   "Analyzing the impact of competence-based approaches on liberal education." Pp. 160–199 in Gerald Grant et al., On Competence: A Critical Analysis of Competence-Based Reforms in Higher Education. San Francisco: Jossey-Bass.
    1979b   "Transforming a liberal arts curriculum: Alverno College." Pp. 259–298 in Gerald Grant et al., On Competence: A Critical Analysis of Competence-Based Reforms in Higher Education. San Francisco: Jossey-Bass.
Gamson, Zelda.
    1979   "Understanding the difficulties of implementing a competence-based curriculum." Pp. 224–258 in Gerald Grant et al., On Competence: A Critical Analysis of Competence-Based Reforms in Higher Education. San Francisco: Jossey-Bass.
Grant, Gerald.
    1979   "Prologue: implications of competence-based education." Pp. 1–17 in Gerald Grant et al., On Competence: A Critical Analysis of Competence-Based Reforms in Higher Education. San Francisco: Jossey-Bass.
Grant, Gerald, Peter Elbow, Thomas Ewens, Zelda Gamson, Wendy Kohli, William Neumann, Virginia Olesen, and Davis Riesman.
    1979   On Competence: A Critical Analysis of Competence-Based Reform in Higher Education. San Francisco: Jossey-Bass.
Habermas, Jurgen.
    1970   Toward a Rational Society: Student Protest, Science, and Politics. Boston: Beacon Press.
Levine, Arthur.
    1978   Handbook on Undergraduate Curriculum. San Francisco: Jossey-Bass.
Marcuse, Herbert.
    1964   One-Dimensional Man. Boston: Beacon Press.
Mills, C. Wright.
    1959   The Sociological Imagination. New York: Oxford University Press.

Contemporary Sociological Theory
Margaret M. Poloma, University of Akron

288 pages/paperback   *ISBN 0-02-396100-7*   *1979*

Many of our current sociological theories will be tomorrow's classic works. *Contemporary Sociological Theory* is a guide to reading the major sociological theories of the past ten years.

Each chapter presents one significant theorist, with an introduction to place the theorist in perspective, synopses of his/her major works, and a discussion of the theorist's assumptions.

Current Issues in Marriage and the Family
Second Edition
J. Gipson Wells, Mississippi State University

392 pages/paperback   *ISBN 0-02-425440-1*   *1979*

The necessity of marriage • *The desirability of children* • *Different forms of marriage* • *Cohabitation* • *Abortion and divorce reform* • *Marital fidelity* • *The changing roles of men and women*... are just a few of the significant issues featured in this anthology.

• 26 fascinating articles (**10 new to this**

SECOND EDITION) drawn from contemporary magazines and professional journals reflect various opinions for each issue presented.

• There is a general introduction to the text and introductions preceding each selection.

Families: Developing Relationships
Mollie Stevens Smart and Laura S. Smart with contributions by
Russell Cook Smart and Craig M. Szwed

527 pages/hardcover   *ISBN 0-02-412020-0*   *1976*

Reviewers agree that this text is beautifully written, well organized, complete, and honest. It provides an interdisciplinary, developmental,

and cross-cultural examination of the various aspects of marriage and family relations. An INSTRUCTOR'S MANUAL is available.

Aging and Old Age
An Introduction to Social Gerontology
Beth B. Hess, County College of Morris, Dover, N.J.
Elizabeth W. Markson, Boston University

386 pages approx./hardcover   *ISBN 0-02-354100-8*   *1980*

There are issues in the study of aging of even broader significance than the plight of old people today: *To what extent is aging biological? To what extent is it socially conditioned? What can we learn about our values and culture from our policies toward the aged? What in our*

society today will affect the way future generations adapt to old age?
These issues and others are explored in depth in this innovative new text about aging and old age.

Communities
A Survey of Theories and Methods of Research, *Second Edition*
Dennis E. Poplin, Murray State University

334 pages/hardcover   *ISBN 0-02-396160-0*   *1979*

Some important debates have arisen from the process of analyzing the community—this text introduces these debates.
It begins by exploring communal forms;

it then gives detailed presentations of major theories of communities; and it covers important studies of community life and methods of community research.

If your students aren't excited about

An Introduction to Sociology

SECOND EDITION

R. Serge Denisoff and Ralph Wahrman

both, Bowling Green State University, Ohio

589 pages/paperback　　　　　*ISBN 0-02-328400-5*　　　　　*1979*

- Is marriage bad for women's mental health?
- Can psychiatrists tell who is or isn't insane?
- Does TV cause violence?

These are only a few of the "provoke-your-students" topics in this introductory sociology text—

An Introduction to Sociology, 2e is...

Straightforward...
- Written by and for people who teach
- With important points clearly presented
- No cryptic jargon
- Clear definitions of terms
- Excellent discussions, footnotes, and end-of-chapter summaries and glossaries.

Up-to-date...
- Illustrates points with timely—often attention-grabbing—examples·
- Discusses *sex role socialization, working women,* etc.
- Has recent material on pop culture and cross-cultural perspectives on minorities.

Comes with an **Instructor's Manual** and **Student Study Guide**.

1. TITLE OF PUBLICATION	A. PUBLICATION NO.	2. DATE OF FILING
The American Sociologist	0 0 0 3 1 2 3 2	9/30/79

3. FREQUENCY OF ISSUE	A. NO. OF ISSUES PUBLISHED ANNUALLY	B. ANNUAL SUBSCRIPTION PRICE
Quarterly	4	$12.00

4. LOCATION OF KNOWN OFFICE OF PUBLICATION (Street, City, County, State and ZIP Code) (Not printers)

1722 N Street, NW, Washington, DC 20036

5. LOCATION OF THE HEADQUARTERS OR GENERAL BUSINESS OFFICES OF THE PUBLISHERS (Not printers)

1722 N Street, NW, Washington, DC 20036

6. NAMES AND COMPLETE ADDRESSES OF PUBLISHER, EDITOR, AND MANAGING EDITOR

PUBLISHER (Name and Address)

American Sociological Association, 1722 N Street, NW, Washington, DC 20036

EDITOR (Name and Address)

Allen Grimshaw, Inst. for Social Research, 1022 E. 3rd St., Bloomington, IN 47401

MANAGING EDITOR (Name and Address)

7. OWNER (If owned by a corporation, its name and address must be stated and also immediately thereunder the names and addresses of stockholders owning or holding 1 percent or more of total amount of stock. If not owned by a corporation, the names and addresses of the individual owners must be given. If owned by a partnership or other unincorporated firm, its name and address, as well as that of each individual must be giving. If the publication is published by a nonprofit organization, its name and address must be stated.)

NAME	ADDRESS
American Sociological Association	1722 N Street, NW Washington, DC 20036

8. KNOWN BONDHOLDERS, MORTGAGEES, AND OTHER SECURITY HOLDERS OWNING OR HOLDING 1 PERCENT OR MORE OF TOTAL AMOUNT OF BONDS, MORTGAGES OR OTHER SECURITIES (If there are none, so state)

NAME	ADDRESS
None	

9. FOR COMPLETION BY NONPROFIT ORGANIZATIONS AUTHORIZED TO MAIL AT SPECIAL RATES (Section 132.122, PSM)

The purpose, function, and nonprofit status of this organization and the exempt status for Federal income tax purposes (Check one)

[X] HAVE NOT CHANGED DURING PRECEDING 12 MONTHS   [ ] HAVE CHANGED DURING PRECEDING 12 MONTHS   (If changed, publisher must submit explanation of change with this statement.)

10. EXTENT AND NATURE OF CIRCULATION	AVERAGE NO. COPIES EACH ISSUE DURING PRECEDING 12 MONTHS	ACTUAL NO. COPIES OF SINGLE ISSUE PUBLISHED NEAREST TO FILING DATE
A. TOTAL NO. COPIES PRINTED (Net Press Run)	4,500	4,550
B. PAID CIRCULATION 1. SALES THROUGH DEALERS AND CARRIERS, STREET VENDORS AND COUNTER SALES	None	None
2. MAIL SUBSCRIPTIONS	3,500	3,704
C. TOTAL PAID CIRCULATION (Sum of 10B1 and 10B2)	3,500	3,704
D. FREE DISTRIBUTION BY MAIL, CARRIER OR OTHER MEANS SAMPLES, COMPLIMENTARY, AND OTHER FREE COPIES	None	None
E. TOTAL DISTRIBUTION (Sum of C and D)	3,500	3,704
F. COPIES NOT DISTRIBUTED 1. OFFICE USE, LEFT OVER, UNACCOUNTED, SPOILED AFTER PRINTING	1,000	846
2. RETURNS FROM NEWS AGENTS	None	None
G. TOTAL (Sum of E, F1 and 2—should equal net press run shown in A)	4,500	4,550

11. I certify that the statements made by me above are correct and complete.	SIGNATURE AND TITLE OF EDITOR, PUBLISHER, BUSINESS MANAGER, OR OWNER *Alice F. Myers* Alice F. Myers Administrative Officer

12. FOR COMPLETION BY PUBLISHERS MAILING AT THE REGULAR RATES (Section 132.121, Postal Service Manual)

39 U. S. C. 3626 provides in pertinent part: "No person who would have been entitled to mail matter under former section 4359 of this title shall mail such matter at the rates provided under this subsection unless he files annually with the Postal Service a written request for permission to mail matter at such rates."

In accordance with the provisions of this statute, I hereby request permission to mail the publication named in item 1 at the phased postage rates presently authorized by 39 U. S. C. 3626.

SIGNATURE AND TITLE OF EDITOR, PUBLISHER, BUSINESS MANAGER, OR OWNER
Alice F. Myers Alice F. Myers Administrative Officer

ROSE MONOGRAPH SERIES

(Published by Cambridge University Press)

(Suzanne Keller, Editor)

The ASA Rose Monograph Series, established in 1968 by a fund set up by Arnold and Caroline Rose, was developed to publish high quality monographs and theoretical studies in sociology. Titles include:

Middle Start: An Experiment in the Educational Enrichment of Young Adolescents
(Yinger, Ikeda, Laycock, Cutler) (Cloth—$13.95; Paper—$4.95)*

Education, Employment and Migration: Israel in Comparative Perspective
(Ritterband) (Cloth—$13.95; Paper—$4.95)*

Class, Race and Worker Insurgency: The League of Revolutionary Black Workers
(Geschwender) (Cloth—$13.95; Paper—$4.95)*

Protest and Participation: The New Working Class in Italy
(Low-Beer) (Cloth—$16.95; Paper—$4.95)*

Continuity and Change: A Study of Two Ethnic Communities in Israel
(Simon) (Cloth—$13.95; Paper—$4.95)*

Cities with Little Crime: The Case of Switzerland
(Clinard) (Cloth—$13.50; Paper—$4.95)*

Opening and Closing: Strategies of Information Adaptation in Society
(Klapp) (Cloth—$13.95; Paper—$4.95)*

Tasks and Social Relationships in Classrooms: A Study of Instructional Organization and Its Consequences
(Bossert) (Cloth—$14.95; Paper—$4.95)*

Juvenile Delinquency and Its Origins: An Integrated Theoretical Approach
(Johnson) (Cloth—$14.95; Paper—$4.95)*

Understanding Events: Affect and the Construction of Social Action
(Heise) (Cloth—$14.95; Paper—$4.95)*

From Student to Nurse: A Longitudinal Study of Socialization
(Simpson) (Cloth—$14.95; Paper—$4.95)*

Order the above monographs from: CAMBRIDGE UNIVERSITY PRESS, 32 East 57th Street, New York, New York 10022

Books previously published in the series . . . Available only in paperback at $5.00 each from the ASA, 1722 N Street, N.W., Washington, D.C. 20036:

Socioeconomic Background and Educational Performance (Hauser) • *Black and White Self-Esteem: The Urban School Child* (Rosenberg & Simmons) • *Looking Ahead: Self-Conceptions, Race and Family as Determinants of Adolescent Orientation to Achievement* (Gordon) • *Black Students in Protest: A Study of the Origins of the Black Student Movement* (Orum) • *Attitudes and Facilitation in the Attainment of Status* (Gasson, Haller & Sewell) • *Patterns of Contact with Relatives* (Klatzky) • *Interorganizational Activation in Urban Communities: Deducations from the Concept of System* (Turk) • *The Study of Political Commitment* (DeLamater) • *Ambition and Attainment: A Study of Four Samples of American Boys* (Kerckhoff) • *The Greek Peasant* (McNall) • *Patterns of Scientific Research: A Comparative Analysis of Research in Three Scientific Fields* (Hargens) • *Ethnic Stratification in Peninsular Malaysia* (Hirschman) • *Deviance, Selves and Others* (Schwartz & Stryker)

*Individual members of the A.S.A. are entitled to a special discount. Contact:

AMERICAN SOCIOLOGICAL ASSOCIATION
1722 N Street, N.W.
Washington, D.C. 20036

Notice to Contributors

Preparation of Copy

The Publications Committee of the ASA, at its August 1975 meeting, passed some new guidelines for journal contributors and editors, thus necessitating several changes in this Notice. Please read the following instructions very carefully before submitting a manuscript.

1. Submit FIVE copies of each manuscript. (If a manuscript is accepted for publication, the editor may request additional copies from the author.) Each copy should have a title page with the author's name and institutional affiliation. To withhold a contributor's identity from the reviewers, give name of author on title page only. Manuscripts will not be returned, so retain a copy for your own files. Enclose a self-addressed postal card, with ms. title, for acknowledgment of ms. receipt.

2. Submit FIVE copies of a 100–150 word abstract with each manuscript submitted. The abstract should be on a separate page, double-spaced, with no identifying references to the author. At the end of the abstract, list one or two index headings which might be used for the article.

3. Type all copy—including footnotes, indented matter and reference—double spaced, on white standard paper. Each line must not exceed six inches. Left-hand margin must be 1½ inches; all others, 1 inch.

4. Type each table on a separate page. Insert a location note, e.g., "Table 2 about here," at an appropriate place in the text.

5. Type footnotes (double spaced) on separate sheets, never in the body of the text. They should not include identifying remarks, or grant numbers and/or credits, which should appear at the foot of the title page with asterisk.

6. Draw figures with India Ink on white paper. Attach these to each copy of the manuscript, but retain the original drawing for direct mailing to the editorial office upon acceptance of the ms. for publication.

7. Clarify all symbols with words in the margin of the ms. Encircle these and other explanatory notes not intended for printing.

8. Authors will be notified when the ms. is accepted for publication. An edited copy will be sent to the author for final corrections, and should be returned as promptly as possible to the Editorial Office. Authors will no longer receive proofs for correction. Both galley proofs and page proofs will be read at the Editorial Office only.

Format of References in Text

All references to monographs, articles, and statistical sources are to be identified at an appropriate point in the text by last name of author, year of publication, and pagination where appropriate, all within parentheses. Footnotes should be avoided. There is no need for "Ibid.," "op. cit.," or "loc. cit."; specify subsequent citations of the same source in the same way as the first citation. Examples follow:

1. If author's name is in the text, follow it with year in parentheses. ["... Duncan (1959) has proven that ..."] If author's name is not in the text, insert at an appropriate point the last name and year, separated by comma. ["... some have claimed (cf. Gouldner, 1963) that ..."]

2. Pagination (without "p." or "pp.") follows year of publication, separated by colon. ["... it has been noted (Lipset, 1964:61–4) that ..."] Incorporate within parentheses any brief phrase associated within reference. ["... have claimed that this is so (but see Jones, 1952:99 for conflicting view.)"]

3. With dual authorship, give both last names; for more than two, use "et al." within the text; full names should be included in the "REFERENCES" section. For institutional authorship, supply minimum identification from the beginning of the complete citation. ["... occupational data (U.S. Bureau of the Census, 1963:117) reveal ..."]

4. If there is more than one reference to the same author and year, distinguish them by use of letters (a, b) attached to year of publication, in text and in reference appendix. ["... as was previously suggested (Levy, 1965a:331) ..."]

5. Enclose a series of references within a single pair of parentheses and separate by semicolons. ["... as many have noted (Johnson, 1942; Perry, 1947; Linquist, 1948) ..."]

Format of References in Appendix

List all items alphabetically by author and, within author, by year of publication, in an appendix, titled "REFERENCES." Use no italics and no abbreviations. Spell out first and last names. When citing a work in other than its original published version, give date of original publication in parentheses. For typing format, see the following examples:

Durkheim, Émile
    1964 The Division of Labor in Society. New York: The Free Press of Glencoe. (Originally published in 1893.)
Smith, Dorothy
    1974a "Social construction of documentary reality." Sociological Inquiry 44:257–267. [Notice lower case letters used in titles.]
    1974b "The ideological practice of sociology." Catalyst 8:39–54.
Wrong, Dennis
    1963 "The oversocialized conception of man in modern sociology." Pp. 68–79 in Neil J. Smelser and W.T. Smelser (eds.), Personality and Social Sysems. New York: Wiley.

ASA Multiple Submissions Policy: Submission of a manuscript to a professional journal clearly implies commitment to publish in that journal. The competition for journal space requires a great deal of time and effort on the part of editorial readers whose main compensation for this service is the opportunity to read papers prior to publication and the gratification associated with discharge of professional obligation. For these reasons, *the American Sociological Association regards submission of a manuscript to a professional journal while that paper is under review by another journal as unacceptable.*

Revised 1978 TAS

THE ASA AT 75

SHOOT THE PIANO PLAYER?—
S/HE'S DOING THE BEST S/HE CAN!!
(or is s/he?)

The American Sociological Association has survived for 75 years as an association formed of, and for, persons sharing "sociological interests." It has attempted (wisely and well/ unwisely and poorly) to represent sociology as a discipline and profession to others. Our 75th year is a propitious time to assess what the Association has become and what it may have failed to become. How well does it meet our various objectives, issues, and concerns? *The American Sociologist* seeks papers that:

- provide insight into the development of ASA as we now know it
- assess particular ways that ASA (responds/does not respond) to professional and disciplinary needs
- analyze ASA's relationship to other associations and institutions (social, educational, governmental)
- offer proposals for changing ASA's mode of operations and activities
- critique ASA's role with respect to changes in American and other societies.

꙾

DEADLINE FOR SUBMISSION OF PAPERS: *August 1, 1980*. CONTACT: James L. McCartney, Editor / *The American Sociologist* / Department of Sociology / University of Missouri / Columbia MO 65211.

ne American Sociologist

Volume 15 Number 2 May 1980
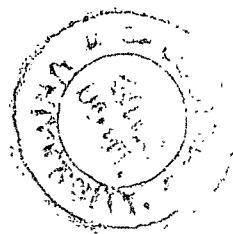
EDITOR'S PAGE: ON IMMORTALITY AND CROCUSES

Quite without planning it, as we assembled the papers for this Spring issue during the late gray days of winter, a theme popped up. The mortality of sociologists and the immortality of their ideas are, respectively, sobering and hopeful topics. However grand or modest our vision of social reality, probably each of us hopes to be remembered for some contribution to the profession. For most of us, the recognition will be in the form of an isolated citation, buried in the appendix of an overzealous Ph.D. candidate's dissertation. For some, the legacy will be greater, and for a special few, the legatees will recognize that they truly understand more as a result of the person's contribution.

In this issue the works of several persons are examined and assessments offered about the potential significance of their ideas. The death of Talcott Parsons prompted numerous tributes to a man who made innumerable scholarly and professional contributions, including the initial editorship of TAS. As Alvin Gouldner observed in *The Coming Crisis of Western Sociology*, Parsons served for many as the intellectual anchor of academic sociological theory in the modern world.

Although TAS typically has not published obituaries, the import of Parsons's work led us to decide to publish the comments offered by his former students at the ASA Memorial Session. By publishing these comments, we hope to inspire reflections on, and scholarly evaluations of, the past, present, and future significance of Parsons's ideas. Whether TAS or other journals are the publishers of this scholarship, the profession will be richer for the

opportunity to consider in what ways Parsons's death is a watershed.

Two other papers in this issue cast the theme of the immortality of ideas in different lights. Faught's paper on Everett C. Hughes shows the continuity in Hughes's work with the Chicago School paradigm. Faught contends that the concept of "schools" of thought helps account for the development of a major scholar's work.

Chua's paper on Michel Foucault argues that Foucault's writing may fundamentally alter sociological investigations. Foucault's work on discursive formations and practices provides not only a critique of contemporary sociological theories, but an exemplar for future sociological investigation.

The theme of immortality is rounded out nicely by a provocative paper by Westie and Kick on retired sociologists' expectations for professional immortality. The identity "sociologist" is a sticky one; despite retirement, many sociologists expect that their research and writing will outlive them. In fact, upon retirement, the sociologists in Westie and Kick's sample are even more optimistic that they will be remembered as leading contributors to their specialties than are actively employed sociologists.

Will the aspirations of this sample, and the rest of us, be realized? Will our contributions to the profession be acknowledged? With all due modesty, considering the conventions of scholarship, it is likely. As the several papers in this issue testify,

The American Sociologist

Volume 15 Number 2 May 1980

For information for contributors, see TAS 15(1, February), inside back cover.

◆

TALCOTT PARSONS, 1902–1979
THE MAN AND HIS WORK

MEMORIAL SESSION OF THE AMERICAN SOCIOLOGICAL ASSOCIATION
AUGUST 28th, 1979*

ROBERT K. MERTON
PRESIDING

I speak for all of us in thanking Tad Blalock and his associates of the Program Committee for having set aside this time to celebrate the memory of Talcott Parsons. Talcott would have been the first to recognize that during this hour of commemoration we transpose this place from the realm of the secular to the realm of the sacred.

At the memorial service held in the Harvard Chapel last Spring and briefly in this room last night, students from Talcott's later years told their recollections of him. Tonight, five of us who were his students a good many years ago will pay tribute to his work and some will tell what it was like to have studied with him in those early years. We do so in the thought that the age distribution of our sociological community being what it is, relatively few of us could have known that young man.

We shall be speaking in the order of recency of our first encounter with Talcott Parsons as our teacher. So it is that the first of us to speak is Robert Bellah, of the University of California, who studied with Talcott as an undergraduate at Harvard in the late 1940s and as a graduate student in the early 1950s. In what amounts to a sociological prose-poem, he will trace the complex development of Talcott Parsons's ideas over the decades.

---

* Throughout, the introductory remarks in italics are those of Robert K. Merton.

THE WORLD IS THE WORLD THROUGH ITS THEORISTS—
IN MEMORY OF TALCOTT PARSONS*

Robert Bellah

One of Wallace Stevens's aphorisms helps us understand Talcott Parsons: "The world is the world through its theorists. Their function is to conceive of the whole and, from the center of their immense perspectives, to tell us about it." And that is, I think, what Talcott did. Unlike any other social scientist of his generation, or philosopher either, for that matter, he conceived of the whole and, from the center of his immense perspectives, he told us about it. Talcott's greatness rests in his intention to encompass the whole, an intention that links him to the seminal minds in philosophy, religion, and poetry of all ages.

We sometimes think of Talcott as the quintessentially sociologist's sociologist, and so in a way he was. His thought about human beings was rooted in his conception of the social as always involving a normative order, that minimal basis of shared moral meanings without which any sustained interaction between men becomes impossible. But Talcott's notion of the social system, so central to his work in the middle phase, was never narrowly sociological. He saw that the social system embraces politics, economics, and religion and that even being sociological in the strict sense means crossing disciplinary boundaries into economics, political science, law, and religious studies all the time. But Talcott also saw from fairly early on that the social system was only

one of the several great organizing foci of human action and that one also had to talk about the cultural system, the personality, and the behavioral organism. Only a sense of the interaction and interpenetration of these four systems could begin to render accurate an understanding of human existence. Perhaps his conception of the four sub-systems of action is still his most enduring legacy to us. He gave us a way of keeping in mind the major components of human action in a way that neither debased nor exalted the sociological, the psychological, the cultural, or the organic. Many current disputes in philosophy, history, and political theory as well as social science more narrowly conceived would be resolved by a proper, jargon-free, application of Talcott's basic insights.

But Talcott was not content to stop with the action system. Toward the end of his life he turned more and more to a consideration of what he came to call the human condition paradigm. In the human condition paradigm, the action system became only one of the four sub-systems, the others being the physical, the biological, and what he called, borrowing from Kant, the telic system, the system of ultimate ends. How fruitful the human condition paradigm might be remains to be seen. Talcott lived long enough only to sketch its outlines. It remains for those of us who were working on it with him to develop further its implications. But what it shows clearly enough is the intentionality in Talcott's mind to embrace the whole, to leave nothing out, to know that we know nothing particular unless we have a glimpse of the whole even though we can only glimpse the whole when we have surveyed the parts. We can see his life work as a series of ever widening circles and the basic telos of his thought to integrate, integrate, integrate more and more reality.

Talcott spoke of general theory and he liked to use the analogy of Newtonian physics. Certainly when I first knew him it was the model of natural science that he was attempting to follow in developing his comprehensive sociology. Though a life-long opponent of the positivists in his conception of human action, he seemed to share with them the ideal of an abstract content-free theoretical science from which one could deduce the laws of social life. But those who took him at his word in this project were bitterly disappointed. His work remained, as he often said himself, at the level of a conceptual scheme, the necessary phase, he said, before the construction of a tight theoretical system from which testable hypotheses could be deduced. And so the empiricists attacked him time and again on the grounds that his ideas remained vague, untestable, incapable of empirical disconfirmation. What they did not know and what Talcott only gradually and obscurely discerned is that in the human studies there is not ever going to be a tight deductive theoretical system, that all we will ever have are analogies and metaphors that direct us to look in new ways at social reality, that Talcott's level of the conceptual scheme, even at its most outlandishly analogical and metaphorical, is not preliminary to anything but is sociology itself.

There is another model of social science that I think was implicit in Talcott's work from the beginning and that casts his achievement in a very different light. This is the hermeneutic or interpretive model that insists that in the human studies there is no such thing as a context-free theoretical science, that everything we do comes out of a tradition already laden with prejudgments and preconceptions and that what we learn is because of that tradition and its preconceptions and not in spite of it. However much he may have admired the model of theoretical physics, Talcott always knew, to use the jargon of our day, "where he was coming from" historically. His first and in some respects his greatest book, *The Structure of Social Action,* is a painfully careful grounding of his own thought in a particular tradition or set of traditions in modern European intellectual history. In true hermeneutic fashion he read his sources with his own problems in mind so that what resulted was a fusion of his horizon with the horizons of Durkheim, Weber, and Marshall. Intellectual history was for Talcott never antiquarianism—it was always a way of developing and extending the tradition even while learning from it and letting himself be criticized by it. And as any

inspection of his bibliography will show, he returned again and again to the sustenance of his tradition in the development of his own thought. In fact I do not believe there was a day in his professional life when Talcott did not have a conversation with Durkheim or Weber, and usually with both.

His final essay on the human condition paradigm is particularly conscious of his place in the whole tradition of Western philosophy, and of his indebtedness to Kant in particular. And in his essays on Christianity and on the evolution of society, he roots himself not only in the cognitive tradition of the West but in the political, moral, and religious tradition as well. Jeffrey Alexander's brilliant memorial to Talcott in the June 2, 1979, issue of *The New Republic* situates him as a liberal social thinker in the deepest Western and American sense of the word liberal.

What I mean when I call Talcott a hermeneutical sociologist is that I think what he finally gave us was not a general science of society or even a prolegomenon to one, but rather an *interpretation* of our current reality rooted in a particular tradition. Though aspiring to wholeness and truth, he never claimed more than that glimpse which even the greatest thinkers are limited to giving us. Indeed on one of the last occasions I had to enter into a discussion with him, I asked him whether he did not now feel that the natural science model had been misguided. He replied that, in the form I first heard it in the early 1950s, he had indeed long aban-

doned it and that it would be possible to view even the most formalistic aspects of his work as only heuristic devices to assist interpretation.

This is not the moment, perhaps not even the generation, to sum up Talcott's achievements and to separate what is his enduring legacy from those efforts which can safely be ignored. It is a tribute to his enormous fecundity that it will probably be a long time before we can even begin to be sure of that sorting. In the meantime there will be a contest over what his thought means and I am fully aware that I have entered into that contest here. I have seen him rooted in particularity and as aspiring always for a broader glimpse of the whole. I have presented him as seeing social science finally, not as a set of testable hypotheses derived from a neutral context-free theory, but as a set of interpretations trying to make sense of reality from the point of view of quite specific intellectual and moral commitments.

We have only begun to learn from him. I think he shows us more of ourselves than any American mind since John Dewey. He was, to use Wallace Stevens's words again,

> The impossible possible philosopher's man,
> The Man who has had the time to think enough,
> The central man, the human globe, responsive
> As a mirror with a voice, the man of glass,
> Who in a million diamonds sums us up.

* * * * *

Jesse Pitts, of Oakland University, received his A.B. from Harvard in 1941, returned to complete his Ph.D. in the mid-1950s, and in 1961, collaborated with Talcott Parsons on the two-volume Theories of Society. *Jesse will speak about the values, the morality, and the public actions of this quietly public man.*

TALCOTT PARSONS: THE SOCIOLOGIST AS THE LAST PURITAN

Jesse R. Pitts

Talcott Parsons will probably take his place among the very great of our discipline, alongside Durkheim, Pareto, Simmel, and Weber. Yet, it is unlikely that any of these great men had to endure in their lifetime so much incomprehension, so much abuse. And it hit Parsons at a time when he seemed to be at the height

of his academic fortunes, since he had become the Chairman of the Department of Social Relations at Harvard University, *primus inter pares,* when the *pares* were such strong personalities as Henry Murray, Clyde Kluckhohn, and Gordon Allport.

In 1947–1948, he was even accused of being anti-Semitic because he defended the legitimacy of showing a movie (I believe it was either "The Merchant of Venice" or a movie about Martin Luther), which some Jewish students found offensive. Five years later, his defense of a young colleague accused of communist affiliation and his championing of Oppenheimer got him into trouble with the U.S. Government. There are always the cowardly among civil servants who will curry favor or simply cover themselves *vis-à-vis* whatever mass movement seems to be popular. At that time it was the McCarthyite movement. When Parsons was in England during the years 1953–1954, there was talk of lifting his passport.

In those days you had in Academe two basic types of supporters for the victims of McCarthyism. These were few and usually had the unanimous support of their colleagues, most often latent support, but sometimes, as in the case of Parsons, overt support with no thought for personal risk. First, you had in Academe former fellow-travellers who had found in some secret and playful allegiance to Soviet Russia a reinforcement of their alienation, a barrier against the dissolving of their marginality to a society in which they were so comfortable that they feared for their intellectual creativity. And now this secret allegiance, this submission to the leadership of Joseph Stalin, was declared to have had a flavor of treason, since a few were suspected of having followed the logic of this intellectual game to its ultimate conclusion. For those, the innocence of a Hiss, of an Oppenheimer, was their own innocence. By defending those two, among others, they were in reality defending themselves.

And then you had the more numerous others who, like Parsons, were convinced of their fellow intellectuals' innocence simply because they could not believe in the deviousness of radical politics. People were to be taken at face value: they were what they said they were, what they wanted to be. If they said they were innocent, they were innocent. And indeed, they were typically no more "guilty" than the many more who, today, are accused of racism and sexism.

This basic trust in people one hardly knows, this refusal to suspect the worst about the stranger, is one of the more admirable traits of the Puritan character. Parsons exemplified to a high degree what some would call a naïvete about human nature that had survived, in his case, a great deal of exposure to Freudian thought. It is not the naïvete of self-righteousness, for it is often accompanied, in its finer incarnations, with much questioning of one's own worth and sincerity. Let me give you an instance of what I mean: In the late 1950s, Professor Sorokin circulated a letter charging Talcott Parsons with plagiarism, especially in the matter of the concept of Immanent Causation.

I remember how upset and distraught Parsons was about this accusation. And he was distressed because, in a corner of his mind, he felt guilty. He knew he had not plagiarized, nor had he been influenced by Sorokin in the development of his own theories. At most he had suffered a lapse in scholarship: he had not read Professor Sorokin's work with sufficient attention, and so had remained unaware of yet another phenomenon of intellectual convergence. But there was also the guilt of having dethroned, in an unwitting and affectively neutral way, a scholar who had had, at one time, his own moment of truth.

As a result, when we published *Theories of Society* a few years later, we included Sorokin's writings on Immanent Causation, to atone for a sin that Parsons had not really committed. And when Professor Tiryakian approached him diffidently about the possibility of contributing to the Sorokin *Festschrift*, he found that his instinct about the man had been right. Not only did Parsons agree to contribute, but he did not, for that purpose, empty out an old drawer or dash off something with the left hand. Instead he contributed one

of the best papers in a book destined to honor a person who had wounded him so grievously, so unjustly, and, true enough, so inevitably. Such was the nobility of Parsons's character.

But fate had harder blows in store for Parsons. I shall not speak here of the personal tragedies, the deaths that struck his heart, for these are the inevitable components of the human condition.

I shall limit myself to the scientific tragedies which afflicted him in the 1960s and 1970s, for, if the truth be known, Parsons owed much of his fame and most of his detractors to his being utterly misunderstood. On the one hand, there was the fame of the Harvard professor, the unwitting beneficiary of our rituals and monkeyshines, the author of hard-to-read books which had to be on the desk of the compleat sociologist, but more as decoration than as food for thought. On the other hand, Talcott Parsons's name became a fashionable anathema to all those for whom Sociology was to be the battering ram against an American Society they felt compelled to reject. Since they wished to oppose the realities of their dreams to the realities of inequality, war, and prejudice, they resented an approach to sociology which claimed that there were rather narrow limits to the possibilities of purposive change, due to the resistance of inner structures and the interdependence— often invisible—of their various components. Since they knew how their wishes for a better society, more loose, more egalitarian, more communal, came from a higher moral nature (their own), the ten-dency was to ascribe the intellectual warnings inherent in Parsons's theories to a supposedly meaner nature—less kind, less sharing, in a word, more conservative. Such was the fate of a Stevensonian Democrat who thought one should understand the anatomy of society before one put the knife to it. And it is also why many of you today will be surprised to find that in the matter of McCarthyism, he was a victim rather than a persecutor or a passive spectator.

In our discipline when we say of someone, "He is a nice guy," we often mean to put him down, because we are saying, in effect, that he lacks the aggression which maintains marginality and promotes insights into what is hidden. Parsons was a genuinely kind person, and yet few trends in America escaped this quietest of scholars. Parsons's insights were generated by his deep love for his country, a love at once born of a rootedness in the American past, but even more born of a conviction that America was the country most committed to realizing the pattern variables of Universalism, Achievement, Specificity, and Discipline. He felt it was his duty to contribute all he could to the accomplishment of this goal. He struggled to develop our contacts with Soviet sociologists. He labored to the last to build the City of Science, beyond all frontiers.

There was a certain loneliness about this last Puritan who worked in the midst of incomprehension, hostility, and uncertain renown. But his silence was not cold.

*   *   *   *   *

Robin Williams, Jr., of Cornell University, studied with Talcott Parsons in the late 1930s and received his doctorate from Harvard a few years later. He examines the style and substance of Talcott Parsons's thought as these relate to the character structure of the man.

TALCOTT PARSONS: THE STEREOTYPES AND THE REALITIES

Robin Williams, Jr.

My acquaintance with the person we honor here began in 1938 when I started graduate work at Harvard. Talcott's first major book, *The Structure of Social Ac-* *tion*, had just been published (1937). During 1938–1939 I participated in his courses on "Sociology of Religion" and on "Social Institutions." Both sets of lectures

emphasized comparative-historical analysis. The notes from those classes, retained still in the files, verify my memory of the lectures as not only brilliant and well-organized, but also as filled with numerous concrete illustrations and expressed in simple, clear, straightforward English prose. Over the subsequent years, when it became a fashionable cliche to bemoan the alleged difficulty and obscurity of Parsonian writings, I often have wished that the critics could have heard the lucid step-by-step analysis that typified those lectures and seminars.

One other observation from those early days is revealing of the man and his work. Talcott already had become quite interested in Freud (although one could find just two citations to Freudian ideas in the entire bulk of *The Structure of Social Action*), and offered to give a series of non-credit afternoon seminars on Freud in relation to the theory of action. This was an add-on to Parsons's teaching but neither he nor his students thought of it as a "teaching load." It was an intellectually exciting enterprise of the highest order. As a teacher, Parsons was able to convey a sense of the drama of discovery, to suggest enticing possibilities of new knowledge to be gained, and to continually stimulate his students to search for hidden regularities and connections in the social world.

Intensive exposure to his mind in action was an experience one was unlikely to forget. Aside from the obvious intellectual power he brought to bear upon an enormous range of ideas and data, two characteristics stand out in my own memory. The first was the intensity of his interests; he had a real passion to know, a driving urge to follow each emerging question to its limits, a ceaseless curiosity, an "incurable" (as he would say) desire to bring conceptual clarity and order out of the chaos of unanalyzed social experience. The second salient characteristic was unrelenting tenacity—a kind of dogged audacity—in seeking out and attempting to resolve the questions that are most fundamental for a sociology that really wants to deal with the human condition. Not for him a science of the college sophomore—a dated and localized account of the tran-

sient aspects of superficial social activities. From the start he was acutely concerned with high-risk intellectual problems in the analysis of basic phenomena: power, conflict, order, change, deviance and control, illness, death, meaning, value, the basis of institutions, the "gift of life."

Nor for him a disembodied phantom of a social actor. On the contrary, the concrete actor is a partially socialized human being who has the organismic characteristics of energy, capacity for learning, dependency and vulnerability, high sensitivity to stimuli and the ability to discriminate and generalize among them, sexuality, the capacity to use symbols and to remember and to anticipate, mortality, and a number of other significant properties that are elicited in the life-course of individuals living in society.

Contrary to the interpretation by some critics, the fact is that Parsons repeatedly and emphatically called attention to strains and inconsistencies within and among the three analytically separable systems of society, culture, and personality. The fundamental image is that of an energized network of interactions among personalities, whose goals and concrete motivations are partly shaped by shared sets of norms and symbols, and who must cope with survival problems *vis-à-vis* the physical environment and the actions of other societies and collectivities. Inherent in social action as he thus saw it are tendencies toward deviance and alienation of individuals, toward inconsistencies in cultural patterning, and toward secession, schism, and conflict in the relations of sub-units of social structure.

Like all great thinkers who continue to grow and to raise new questions, Parsons continually engaged in self-criticism and reworking of his ideas. He produced an enormous body of complex thought that ranged over most of the major concerns of sociology and extended deeply into the neighboring disciplines of anthropology, psychology, psychiatry, economics, and political science. The direction of his work was consistent even as the goal was audacious: the development of concepts and theoretical frameworks capable of ordering all the most important analytic knowledge of human social conduct into inter-

related sets of empirically relevant defini-
tions and propositions. But the complex-
ity of the conceptual analysis, the diver-
sity of the empirical problems treated, and
the changing character (and the changing
social contexts) of the work—all these in-
sure that adequate interpretation of any
major part of the total enterprise requires
extremely careful scholarship and a de-
tailed knowledge of context and devel-
opmental sequence.

Few sociologists of our time have been
more subjected to stereotyping, to care-
less *ad hoc* readings, and to selectively·
distorted interpretations. Anything like a
genuinely scholarly analysis will show
how grossly misleading it is to imply that
Parsons held any simplistic view of
societal "consensus," that he had a
"static theory," that he regarded human
beings as over-socialized cultural robots,
or that, indeed, there ever was any need to
bring concrete living men or women
"back into" his view of the social uni-
verse. For he observed keenly and
thought deeply about the central political
issues and personal concerns of people in

the here and now. His numerous essays
fully document the point—from analysis
of fascist movements and racial discrimi-
nation, to educational policy, to medical
care. He was an alert and shrewd
diagnostician—whether of McCarthyism,
or of psychosomatic disorders, or of the
internal politics of the A.S.A.

He was a man of rectitude and compas-
sion and good taste. He was well ac-
quainted with the tragic side of life, but he
never wore his sorrows in public. He an-
swered criticism rarely; when he did, his
commentaries were utterly free of per-
sonal rancor and he dealt with his oppo-
nents with professional respect, assuming
a common interest in the search for valid
knowledge. In a time of impassioned
ideological controversies, he exemplified
intellectual integrity and steadfast clarity
of aim.

Talcott Parsons was a modest and
realistic man. His achievements were
vast. His contributions are of great and
permanent significance.

*   *   * ·*   *   *

*We move through another temporal stratum to John W. Riley, Jr., who, after a stint
as Chairman of the Department of Sociology at Rutgers University, went on to become
the first Director of the Office of Social Research of the Equitable Life Assurance
Society of the United States. Jack received his M.A. from Harvard in 1933 and his
doctorate there three years later. It is fair to say that Jack and Matilda Riley have had a
closer family relation to Talcott and Helen Parsons over a longer span of years than
any of the rest of us, and it is from that special vantage point that Jack will speak of
them both.*

TALCOTT PARSONS: AN ANECDOTAL PROFILE

John W. Riley, Jr.

I propose to say nothing about the big
theories which have left their indelible im-
print on the sociological community.
Rather I shall give a few anecdotal
glimpses of some of the less well known
personal characteristics of the man behind
the theories. For it is my view that the big
theories were written by an equally big
man.

Matilda and I talked with Talcott and
Helen on the day early last May when
they left for Heidelberg where Talcott was
to be honored on the occasion of the 50th

anniversary of his doctoral degree from
that venerable university. As usual, he
had several unfinished manuscripts in his
bag—notably a paper on how he had, in
his own student days, been influenced by
Max Weber. By all accounts, the Heidel-
berg occasion—replete with the type of
pomp and ceremony to be found only in
German Universities—was a great suc-
cess. Shortly thereafter the Parsons pro-
ceeded to Munich where Talcott was
scheduled to deliver two lectures at the
University in a hall with Max Weber's

bust in the background. He was still working on the paper. As Helen reports the sequence of events, the lectures were enthusiastically received and as he returned to their hotel on the evening of May 8 following the second lecture, Talcott complained of not feeling well. A doctor was summoned, but Talcott died during the night.

If Talcott literally did not die with his boots on—he did so figuratively. He was an indefatigable worker. To use the old term, he was a trooper.

Although relatively small of stature Talcott made a point of keeping himself physically fit. Frequently I worked with him in the woods back of his New Hampshire farm. Apart from the strenuous task of topping a large pine tree—he would climb it as if he wanted to defy gravity and, at the same time, to prove that Darwin was right! Let me tell you a story of his strength.

One summer our two families were on a camping trip in and around Navaho country in the Southwest. Clyde Kluckhohn had arranged for us to join some graduate students who were on an archaeological dig in one of the canyons. The road that led into the canyon was not a road at all. It was a trail across the sand marked only by rock cairns. And so it was that one of the cars became mired, and so it was that we learned the hard way of building a temporary track over the sand with sage brush. The trick was to use two jacks and to get enough brush under the rear wheels to move the car forward in awkward jerks a few feet at a time. It was painfully slow and frustrating. It was also hot work. And not infrequently would Talcott with a mighty lift save time by substituting his back for the two jacks.

Physically, Talcott was a strong man.

Indeed, that was such a memorable experience that I have a clear recollection of the morning after we had gotten out of the dry quick sand. We had spent the night in the guest hogan at a trading post euphemistically called Mexican Water. The first crisis that morning was that a dog had made off with two of the children's shoes, but Talcott quickly ran the mongrel down. The second one was a scene I shall never forget. There was the indomitable

Helen, cooking hash for all hands over an open camp fire, when a chicken with a loud squawk perched on the lip of the frying pan. Whereupon Talcott quipped, but not so Helen could hear, "There's the influence of the London School of Economics for you. Even today Helen insists that it's possible to have a chicken in every pot."

Talcott was a man of great good humor, he was a punster par excellence, he had an enormous sense of fun.

Incidentally, for those of you who don't know the love story of Helen and Talcott, they met at the London School of Economics where they were both graduate students in the late 1920s—the beginning of a romance which was to last for well over half a century. Talcott and Helen were strong believers in the family as a social institution and they practiced their beliefs. They created small family traditions. At the farm, for example, there was apple wine before dinner, but cocktails on Wednesdays, fried eggs for breakfast on Saturdays, a picnic at the State Park on their way back to Cambridge on Sundays, and so on. But it was not just small rituals. Out of their marriage they forged a tremendous strength. Together, with the strong support of two other children, they suffered and finally transcended perhaps the greatest grief that parents can experience—the self-inflicted death of a child.

As a couple they were role models for many of us, and we were the better for it. Talcott was a good and strong family man.

But I must interrupt with one story about theory. I was teaching at Marietta College in the very early 1930s. Talcott's father, an outstanding Milton scholar, was the President, and he was a man with deep reverence for the English language. I remember one morning he stopped me on the campus on my way to class and asked: "Just what were Talcott's classes like at Harvard? I have spent the last two evenings on a manuscript which he proposes to publish entitled *The Structure of Social Action*, or something like that. Is it possible that he lectures in the same way he writes?" I don't think I was too successful in getting out of the Catch 22 situation, but if you read the next to the last paragraph

of the preface to that great book, Talcott acknowledges two debts; one to the committee on research in the social sciences at Harvard and "the other is to my father, President Emeritus Edward S. Parsons of Marietta College, who took upon himself the heavy burden of going through the whole manuscript in an attempt to improve its English style. Whatever readability an unavoidably difficult work may possess is largely to be credited to him."

The record is clear. Not only did Talcott's father obviously not edit the Preface to *The Structure of Social Action*, but by the time President Parsons had finished editing the text it was time for him to retire!

Rarely did Talcott show any signs of losing his cool—and to the best of my knowledge, he never did, at least in public. The one exception occurred during a memorable and highly private evening at the farm. It was shortly before his formal retirement from Harvard and we were discussing the relationship between academic tenure and the principle of mandatory retirement. Talcott had been badly wounded by the then Harvard rules. Indeed he was outraged. I shall never forget him leaping from his chair and fairly shouting: "How can people continue to talk about tenure as if it were a life-time phenomenon? I am *no longer* a professor at Harvard!" It took us ten minutes to change the subject.

He rose, however, above the bitter and often acerb criticisms of his academic and professional associates. He invariably answered them point by point. He took criticism seriously, and he was an even-tempered man.

And sometimes he could make fun of himself in ways that revealed his humility. I recall an incident in which a paper about Parsonian theory had been transcribed by a non-sociological typist. The author had been talking about Parsons's *pattern variables* and the typed script came out to read Parsons's *pet invariables*. Somehow the marvelous typo got to Talcott. He chuckled and said: "Do you suppose that they just might turn out to be invariable?"

Similarly, he loved the following story which I heard for the first time last June at the Center at Stanford when we held a gathering in his honor and memory. It seems that the year Talcott spent at the Center, an international psychiatric meeting was being held in San Francisco. Field trips had been arranged for the participants—mainly to clinics, mental hospitals, research facilities and the like. The Center for Advanced Study in the Behavioral Sciences was on the list. Several bus loads of visiting doctors were, according to the account, royally entertained and as they boarded their buses for the return trip to San Francisco, one visitor was heard to comment: "The people here must be crazier than we thought. I just had a wonderful encounter with a little guy who claimed that he was Talcott Parsons!"

He saw the humor of such stories but he was basically a simple man. Let me conclude with my theme: We are not likely soon again to encounter a man bigger in so many ways as was Talcott Parsons.

*   *   *   *   *

Continuing with the dark backward and abysm of time, I should report that the last commemorative speaker has been a student and then a lifelong friend of Talcott Parsons since 1931.

REMEMBERING THE YOUNG TALCOTT PARSONS*

Robert K. Merton

Talcott Parsons died last May in Munich, full of years, accomplishments,

and honors. As he might himself have said, it is symbolically appropriate that the end should have come in the same city where, almost sixty years before, death had taken his master-at-a-distance, Max Weber.

I shall speak scarcely at all about the Talcott Parsons known to us as the author of sixteen books and some 200 articles, as the editor of another ten volumes or so, and, through this lifework, as the essential invigorator of the classical tradition of sociological thought. Instead I want to tell about the young Harvard instructor back in the early 1930s, a time when he was experiencing the joys of obscurity rather than resisting the distractions of world .fame.

Honoring our Association by her presence, Helen Parsons may recall how it was that I became Talcott's student almost fifty years ago. It was one minor offshoot of a quite implausible event which had a Lowell, then President of Harvard, actually displacing a Cabot with a Russian emigré, Pitirim Sorokin—all this in the course of transforming a venerable Department of Social Ethics into a newfangled Department of Sociology. (The Lowell was, of course, Abbott Lawrence Lowell; the Cabot, Richard Clarke Cabot, who, to complicate matters further, had married into the Lowells.) As it happens, that doubly improbable new Department contained a newly minted instructor in sociology who, having served his four years as an instructor in economics, was destined, at age 29, to serve another five years as instructor before being elevated to the lofty post of assistant professor. Times evidently change in academe too, as Talcott must have noticed in looking back on that long, slow ascent through the ranks.

I do no injustice to Talcott's memory by reporting that of the very first generation of graduate students coming to Harvard to take up their studies in that new department, precisely none came to study with Talcott. They could scarcely have done so for the simplest of reasons: in 1931, he had no public identity whatever as a sociologist. He had published just two articles deriving from his dissertation and these had appeared in the *Journal of Political Economy*, a journal, it is fair to suppose, not much read by undergraduates in sociology bent on deciding where to do their graduate work. True, the year before, Talcott had translated *The Protestant Ethic* into exceptionally clear, direct, and most unTeutonic English prose. But this achievement too would scarcely draw the attention of aspiring young sociologists to him. And now, I do no injustice to Pitirim Sorokin's memory by reporting that although we students came to study with the renowned Sorokin, a subset of us stayed to work with the unknown Parsons.

As students brought up in the ways of American sociology, we soon came to realize that Parsons was wholly marginal to the main streams of that sociology. Amherst, where he had studied as an undergraduate, had no courses in sociology and neither at the London School of Economics nor at Heidelberg where he pursued his graduate studies was he apt to hear anything of American sociology or to read much in it. He had no cause to know, for example, that Durkheim and especially Simmel, if not Weber, had been introduced to American undergraduates throughout the 1920s and into the 1930s by the sociological bible of the time, the Park and Burgess *Introduction to the Science of Sociology*. (As Talcott remarked many years later: "I do not remember having heard [Max Weber's] name either at Amherst or in London.") Such marginality meant, among other things, that he had to discover Weber and, to some extent, Durkheim for himself, rather than being influenced by prevailing interpretations of their work. It tells us much that the young instructor who began on the margins of American sociology would become, for many, its essential theoretical center. And since the sociological education of Talcott Parsons began before the discipline became fully institutionalized, much of what he learned was self-taught. He just may have been the last great autodidact in the American history of sociology.

Long before Talcott Parsons became one of the Grand Old Men of world sociology, he was for an early few of us its Grand Young Man. This began with his first course in theory, Sociology 21, with its long and severely understated title: "Sociological Theories of Hobhouse, Durkheim, Simmel, Toennies, and Max Weber." As we students surely could not know and as I suspect Talcott himself did not know at the time, this was the course

of lectures that would provide him with the core of his masterwork, *The Structure of Social Action* which, in point of fact, did not appear in print until five years after its first oral publication.

During those first few years of the 1930s, Talcott taught wholly by example, not at all by precept. There we were in class, observing him trying to master the art and craft of sociological analysis in the course of helping to develop them. He plainly enjoyed the task of disciplining unruly ideas to bring them, properly subdued, into his emerging system of thought. He never lost the smile of delight with which he would greet a new idea or an old one that seemed the better for having been incorporated into his own work. When he was thinking especially hard, or responding to a question by a confused student, he would look off into the far or middle distance, and his soft-spoken voice would rise slightly in volume and pitch—I should judge about an octave—as he would say reflectively: "It is clear that . . . " or again, "You have a good point, but as you can easily see . . . " We came to recognize this sort of thing as an audible sign that, for the moment not entirely sure of his ground, he was trying to clarify for himself, and incidentally for us, the precise character of what was emerging from the search for the elusive synthesizing idea. He would then continue with the evolving thought and, as confidence grew, his voice would slowly descend that octave or so to its normal pitch and volume.

To witness the young Talcott Parsons wrestle a recalcitrant idea into submission was an experience not soon forgotten. The sentences, formed of thought in evident process of formation, were long, complex, and slow in completion. Our only choice was to hang onto every word; else, we would be lost as the complex argument unfolded. Even so, there was much talk after each class about what our mentor was really driving at. And so some of us induced him to form what we, not he, immodestly called the Parsons Sociological Group. That group met for some years in his tutorial quarters in Adams House—as I remember, in G-34—which inevitably became tagged as the Parsonage. It was there that Parsonsian so-liloquies and monologues were converted into dialogues or even, on occasion, multilogues.

Unlike Max Weber's students in his last year of teaching at Munich, we did not venerate Talcott—possibly because at his tender age, even he was not yet venerable. But we soon acquired an immense, abiding respect for him, as man and scholar. Such was his maturity that, to us, he looked almost the same at 29 or 30 as he was actually to look at 50 or 55. That is not to say, if you'll allow nuance to violate the rule of symmetry, that he looked the same at 50 or 55 as he looked at 29 or 30. We were all younger than we knew back then, but Talcott himself was ageless.

Talcott was both cause and occasion for our taking sociological theory seriously and, no less important, for taking ourselves· seriously. Because our teacher, as a reference figure, accorded us intellectual respect, because *he* took *us* seriously, we, in strict accord with Meadian theory, came to take ourselves seriously. We had work to do. Soon, we were less students than younger colleagues—fledgling colleagues, to be sure, but colleagues for all that.

Talcott Parsons was for us a new sociological voice. But in saying this, I must not mislead. Few of us back then became disciples, uncritical of what we were being told by a teacher of prime importance to us. (We were, after all, graduate students of the difficult and tumultuous 1930s.) Correlatively, Talcott had early made it clear that our current worth and promise were not being gauged by the extent to which we echoed his ideas. Being human and therefore not altogether secure, he was pleased when any of us adopted some of his ideas since that served as a signal, even for so autonomous a man as Talcott, that they just might be of value. But he never expected us to take his thoughts whole, then and there, or much later. To this moment, for example, I remember the grace with which, some thirty years ago, he responded in a forum of this same Association to my mild-mannered but determined criticism of certain aspects of his theoretical orientation.

For Talcott, sociological theory was a

vocation; we have in him a true instance of *Soziologie als Beruf*. To adapt a phrase of Lionel Trilling's, for half a century he "practiced [sociology] with sober diligence." Those early lectures which eventuated in *The Structure of Social Action* were only prelude to the proliferating succession of landmark books and articles. The sociological well never ran dry. The flow of works continued to the very end, if anything at a growing rate, and with undiminished intellectual force. From the beginning, we were all aware of his intensity of commitment to his self-assigned heroic task of fusing and greatly developing the ideas that derived chiefly from Weber and Durkheim and, somewhat later, from Freud. In discharging that moral obligation, he evolved a style of sociological analysis and synthesis that bore his unmistakable signature.

About that immense and incomparable body of published work, Talcott would have wanted me to say to this company what I said directly to him more than once: that the almost feverish pace at which he published the writings which have made beneficiaries of us all might quite possibly have its drawbacks as well. Had fate been as kind to Talcott Parsons as it was to Thomas Wolfe—not the Kandy Kolored journalist Tom Wolfe of the 1960s and 1970s but the novelist Tom Wolfe of the 1930s—then he would have had his sociological Maxwell Perkins, a creative editor of the first class ready to devote himself to quarrying the foundation stones from the mountain of manuscripts. As it is, we must all serve—and, if we persist, in the end gratefully serve—as our own editor-readers engaged in selectively pondering a body of work in theoretical sociology in the grand style that is unmatched in this century by anyone except Talcott's adopted master, Max Weber.

The death of Talcott Parsons marks the end of an era in sociology and it is far from clear when a new era will begin. When it does begin, and whatever its form and substance, it will surely be fortified by the great tradition of sociological thought which he has left to us.

*   *   *   *   *   *

In closing this session, I ask that you stand for a moment of silent tribute to the memory of our teacher, colleague, and friend.

MANUSCRIPTS FOR THE
ASA ROSE SOCIOLOGY SERIES

Manuscripts (100 to 300 typed pages) are solicited for publication in the *ASA Arnold and Caroline Rose Monograph Series*. The Series welcomes a variety of types of sociological work—qualitative or quantitative empirical studies, and theoretical or methodological treatises. An author should submit three copies of a manuscript for consideration to the Series Editor, Professor Suzanne Keller, Department of Sociology, Princeton University, Princeton, NJ 08540.

PRESUPPOSITIONS OF THE CHICAGO SCHOOL IN THE WORK OF EVERETT C. HUGHES*

JIM FAUGHT

Loyola Marymount University

As an alternative to traditional interpretations of the history of sociology, Tiryakian (1979) has proposed that the development of the discipline may be conceived as reflecting the succession of certain dominant "schools." A school is comprised of a founder-leader, followers, and a paradigm. This paper identifies six presuppositions of the Chicago School paradigm set forth in Park and Burgess's Introduction to the Science of Sociology *(1969) and discusses how five of these presuppositions are central to the work of Everett C. Hughes, one of Park's students and a major figure in the continuity of the Chicago School.*

One of the most important tasks confronting the sociology of knowledge is accounting for the structure and growth of academic disciplines. In sociology, the response to this project has usually taken one of two directions: (1) an examination of the contributions of individual scholars; and (2) an identification of key, or unit, ideas that are traced across time. A survey of principal texts which record the history of sociology documents the near universality of these procedures (Martindale, 1960; Coser, 1971; Nisbet, 1966; Turner, 1974). In a recent essay, Tiryakian (1979) questioned whether such traditional approaches to disciplinary growth have not underestimated the impact of the institutional and interactional context on the transmission of scholarly knowledge. In their place he proposes that the growth of sociology, especially since its institutionalization in the late 19th century, may be more adequately understood as the successive history of dominant "schools."

A school is a fundamental agency for transmitting scientific knowledge. As Tiryakian describes, a school is composed of a founder-leader and followers, who share a conviction about the validity of certain conceptual and methodological tenets (1979:217). The interaction between the founder and followers reinforces their resolve to work out the implications of their paradigm (presuppositions, concepts, theory, methodology, empirical testing procedures), even if they are contrary to accepted opinion. The intellectual influence of a school depends both on the resilience of its interaction network as well as the power of its conceptual core to maintain original followers while attracting future proponents. During the early years of a school, when its paradigm is formulated, the interaction between the founder and the initial adherents is crucial for developing a group committed to proselytizing ideas contrary to conventional ways of viewing the world. Once the founder's students begin communicating the paradigm in other institutions, the primary interaction network diminishes somewhat in importance and the ability of the paradigm to be accommodated to different problem areas becomes essential for assuring a school's continuity. This is not to say that continued association of a school's followers, through journal editorship, joint authorship, and invited participation at professional meetings, is not important for maintaining a sense of continuity, if not community, within a school. Nevertheless, a school's paradigm, which would include certain presuppositions, stands or falls largely on its capacity to organize and generate knowledge.

The notion that a paradigm includes presuppositions in addition to its explicit theory and methodology is central to this paper. Presuppositions are defined as:

> . . . those often implicit ontological groundings of a general theory; presuppositions are not intellectual constructs like hypotheses, empirical propositions, and articulated theories. They are the existential as well as metaphysical foundations, the basic definitions of the situation, the basic approaches to reality which are not falsifiable by any rational or empirical means (Tiryakian, 1979:218).

I hypothesize that the degree of continuity in a school's paradigm may be partly inferred from the consistent reliance of individuals, who once formed a primary interaction network, on a paradigm's presuppositions. As a preliminary effort to assess the utility of the notion of "school" in accounting for the growth of sociology, in the following discussion I will demonstrate how selected theoretical and empirical work of Everett C. Hughes is generally consistent with certain presuppositions of the "Chicago School" of sociology (see Carey, 1975; Faris, 1967; Matthews, 1977; Rucker, 1969) as originally formulated by Park and Burgess (1969). This discussion is preceded by some brief remarks on the nature of Hughes's relationship to Robert Park.

EARLY YEARS AT CHICAGO

In any attempt to estimate the impact of the Chicago School paradigm on the discipline, Everett Hughes represents for later generations of sociologists a crucial link with Park's original formulations. Hughes's friendship and association with Park began during graduate school in the mid-1920s and continued until Park's death in 1944. During his graduate education, Hughes attended a number of Park's classes and completed many projects under his direction. Park directed Hughes's dissertation on the institutionalization of the Chicago Real Estate Board. Park also encouraged Hughes as a graduate student to write and submit a paper to the *American Journal of Sociology* on the generalizable findings of his research (Hughes, 1928). In later years,

Hughes frequently expressed his indebtedness to Park for suggestions on topics ranging from economic expansion to individual marginality. After Park's death his papers were edited into three volumes by Hughes.

Hughes left the University of Chicago upon receipt of his doctorate, but he eventually returned and served as chairperson of the Department of Sociology. In 1928 Hughes took a faculty position at McGill University in Canada, and in so doing, doubled the size of the sociology department. He remained at that post until 1938 when he returned to teach at the University of Chicago. In 1961 he left Chicago to join the faculty at Brandeis, and since 1968 Hughes has been associated with Boston College.

The notion of school as it has been developed by Tiryakian suggests that the early members of a school, that is, the founder and students, should be aware of the distinctiveness of their research paradigm. Their methodology (in the more general sense of the term) sets the school apart from others and permits them to develop an identity as a group with a project (mission) to be completed. In the case of the Chicago School, it thus would be important to establish to what extent such a self consciousness existed. Hughes testifies to the general continuity of Chicago sociology since the turn of the century when he points out that "Park gave new impetus" to the earlier orientation toward social reform *and* the theoretical analysis of society (1971:546). He depicts the historical development of the Department of Sociology at the University of Chicago as unique in that it combined Small's interests in the German theoretical tradition and social reform with others particularly concerned with social problems, surveys, and reforms (1971:500). From Hughes's perspective, the earlier years at the University of Chicago were characterized by a diffusely compatible orientation, but lacking organization into a coherent paradigm. However, shortly after the arrival of Park, and through collaboration with Ernest Burgess, an original synthesis was achieved that gave the future members of the Chicago School a more focused perspective.

Although the Park and Burgess text of 1921 had rapidly become the guide for research questions at Chicago, apparently Park's seminal essay on the city, published in 1925 in *The City* (Park et al., 1925), (although first appearing in the *American Journal of Sociology* in 1915), fused these questions in empirical terms to provide a critical stimulus to legitimating what came to be the Chicago School Paradigm. The essay never achieved the general status of the *Introduction to the Science of Sociology* (1921) as the exemplar of the paradigm, but it did demonstrate the applicability of principles that were to be contained in the paradigm. Hughes recalls that the essay on the city stimulated considerable discussion in the Department of Sociology and in other social science departments at Chicago. Thus, he writes:

> Not long after its publication Small called the faculty of the several departments of social science together and proposed that they all work on a common project—the city—and that they start their work at home. With support from a foundation, this became in fact a *programme* (emphasis added) . . . Park was the natural, if never the official, leader of this very energetic movement (1971:546).

Given Small's expression of the need for unanimity in the research program, as well as other statements acknowledging the direction provided to sociology by Chicago sociology (see Braude, 1970), it should prove of value to examine some of the presuppositions of the Park and Burgess text in order to assess their influence on the work of Everett Hughes. Hughes did not simply "reproduce" the Chicago paradigm, rather he specified and expanded some of its implications; still, the Chicago paradigm as set out by Park and Burgess remains an integral part of his sociological perspective.

PRESUPPOSITIONS OF THE CHICAGO SCHOOL PARADIGM

From Park and Burgess's *Introduction to the Science of Sociology* (1969), I have identified six important presuppositions that stand at the core of the Chicago School paradigm. Undoubtedly, these do not represent all presuppositions in the

paradigm, but they do serve as important focal points around which their specific hypotheses about the processes of social life tend to cluster. The six presuppositions probably are of different orders of inclusiveness but for present purposes that issue will not be considered. They are closely related to the ecological and social psychological thrust of subsequent Chicago School research. In this sense the presuppositions orient researchers to the systematic study of objective (external or spacial) and subjective (internal-meaningful and communicable) phenomena. Their unity exists in a methodological intent that holds fast to the proposition that human behavior is amenable to rational inquiry.

Reliance on Park and Burgess's *Introduction* as the single source for extracting the subsequently identified presuppositions is consistent with Tiryakian's contention that a school's success is partially dependent upon the ability of its founder to formulate a general guide for the subsequent research by followers. Furthermore, the importance of the *Introduction* for two generations of sociologists is attested to by Coser (1971:371) and Faris (1967:37). Park, himself, judged the text to be necessary for establishing sociology as a legitimate intellectual discipline, and refused in the second edition to make the book less demanding for introductory students (see Raushenbush, 1979:83–84). While recognizing that Park's understanding and analysis of social processes over the course of his career reflected some modifications of his earlier position, it appears that the 1921 text-reader is sufficiently instrumental in expressing a widely received perspective that reliance on this source is justified.

The *first* presupposition is elemental to the Chicago paradigm. It simply states that the characteristics by which we distinguish persons as human are developed within a group. In other words, the researcher is encouraged to identify mechanisms outside the individual to explain order and disorder in a social setting. Neither consensus nor dissensus depends on the activity of isolated individuals. This presupposition directs attention to a process of fundamental interest to Chicago

School sociologists, namely, social control (see Janowitz, 1978). Questions regarding group membership, marginality, social distance, cultural conflict, and values are immediately linked to this presupposition.

The *second* presupposition is more strictly methodological. The claim is put forward that comparative observations of human communities and societies are generalizable. This presupposition does not deny the uniqueness present in each social act, nor does it disregard the distinctiveness of concrete data. Rather, the emphasis falls on the belief that in similar types of relationships a limited number of patterns are likely to occur within a given culture. It is the patterning of activity in a social setting that makes generalization possible. At first glance, this presupposition might seem to unduly restrict sociological studies to more formalized structures, while denying validity to studies of more fleeting and transitory action, such as might be displayed in crowds, panics, fads, or social movements. Yet, clearly, this was not the case, as is evident in Park and Burgess's discussion of these processes in their text. The reason for their stipulated lack of closure in identifying appropriate subjects for sociological research is that, although sociology was enjoined to study recurrent activity, which could then be generalized, it was also recognized that what appears to be recurrent is itself never completely stable. The recurrent is always in movement—in a process of structuring— even when it appears structured. Thus collective behavior is not to be conceived as qualitatively different from the routine interaction of more stable groups.

The *third* presupposition holds that a community or a society must be examined as a totality. This is consistent with the idea of an organism adapting to its environment. The Chicago School paradigm certainly did not attempt to apply the organic model as it was received from Spencer or any other precursor. But its emphasis on the crucial significance of competition in the struggle for survival led the Chicago School to a concern for identifying the most salient elements in the environment that contributed to reassert-

ing an equilibrium. In this light, ostensibly no single causal factor could take precedence over another in the explanation of social life. Yet, in this presupposition and the way it is worked out there does seem to be an implicit acceptance of the desirability of the tenets of a laissez faire economy (see MacPherson, 1962). For under this economic system, the artificial restraints on mobility typical of more traditional social orders are severed to permit the "natural" competitiveness (mobility, circulation, striving) of the community to assert itself.

The *fourth* presupposition is that the process of change from collective behavior to institutionalization follows a natural history. Through comparative analysis, repeatable sequences of change may be observed. And all action of a given type will follow this sequence.

The *fifth* presupposition argues that over time social processes regulating life tend to involve more conscious activity. This presupposition is most obvious in Park's analysis of the stages of intergroup contact—competition, conflict, accommodation, and assimilation—in which each later stage involves a greater awareness and sharing in the life of another. According to Park, the temporal priority of unconscious factors associated with competition does not necessarily mean that they determine the shape or outcome of the three subsequent stages of intergroup contact. In Park's view, people may be brought together, or forced into antagonism, over questions of value, which are strictly distinct from questions related to the objective grounds for survival. It also should be pointed out that the stages in the sequence that culminate in assimilation are by no means inevitable. Opposition may erupt at any stage and turn into conflict. This cannot be ruled out even when previously conflicting groups have been assimilated. In the latter stage, competitiveness does not disappear, but turns into a struggle for status.

The implications of the Park and Burgess argument on this point are pertinent for understanding the general thrust of their perspective as well as for identifying their influence on the dominant orientation today in research on inequality

in the United States. By conceptualizing groups and interpersonal conflict located in the stages of accommodation and assimilation as a struggle for status, they shift attention from economics to values. Park and Burgess undoubtedly insist on the need to study social conflict, but only within the context of what is assumed to be the pregiven, unconscious, and natural processes of economic competition that inevitably lead to the creation of markets and social and geographic mobility in the general struggle for survival. Park and Burgess, in this sense, have incorporated into their presuppositions a belief in the survival value of competitive individualism as it was manifested in the earlier stages of competitive capitalism. This is the grounding of their theory of assimilation. Park and Burgess's presupposition becomes clear when they categorize classes as accommodation groups, rather than as sources of conflict.

The *sixth* presupposition holds that members of social groups are reflexive. Individuals become reflexive in the process of assigning common meanings to gestures and ultimately communicating with each other as a consequence of an implicit trust that together they share significant symbols. The moral community created in this process is rooted in the competitive struggle for survival, but is not determined by the dynamics of the latter. This presupposition leads to a consideration of the different bases for identity, the creation of roles, the self concept, and one's definition of a situation.

In summary, the six presuppositions identified as central to the Chicago School research paradigm are: (1) the sources of order and conflict are external to the individual; (2) comparative sociological observations are generalizable; (3) social life should be studied as a (competitive) totality; (4) social change follows a natural history; (5) as social aggregates continue to have contact, conscious activity becomes increasingly important; and (6) the reflexivity of group members permits the establishment of a moral community. It is a testimony to the influence of the Chicago School that many of these presuppositions have become incorporated into much of contemporary sociol-

ogy. Hypotheses related to these presuppositions, and some aspects of the presuppositions and their historical setting have been discussed by recent commentators on the work of Park and the Chicago School (see Faris, 1967:37–50; Carey, 1975:95–120, 151–190; Matthews, 1977:121–193; Coser, 1971:357–384). However, this paper attempts to bring together and identify in a more systematic fashion than previously has been available, certain core presuppositions viewed as pivotal to the enterprise of sociology as it was conceived by Park.[1]

EVERETT HUGHES AND THE CHICAGO SCHOOL PARADIGM

The sociological writings of Everett Hughes seem essentially compatible with the substance of these presuppositions.[2] As a student of Park, and one who acknowledges the importance of the Parkian legacy, Hughes's research should reveal a strong affinity with most of the six presuppositions identified above. Furthermore, we might expect that these presuppositions would provide a guide for research in more highly specified domains of inquiry than the proponents of the original paradigm felt constrained to investigate. The paradigm gave clues as to what was to be observed and what was to be ignored in various contexts.

The influence of the Chicago School

---

[1] A comparative study of alternative perspectives to the Chicago School in the 1920s and 1930s is beyond the scope of this paper. Certainly exponents of such notable figures as Veblen, Marx, and Sumner could be found, but their impact on the discipline at that time was not nearly as substantial as the institutionally grounded Chicago School. In this vein, an analysis of alternative presuppositions current at the time of the publication of Park and Burgess's *Introduction to the Science of Sociology* also seemed unnecessary for the present purpose, although such a task might prove enlightening in revealing the "problematic" within which Park developed his ideas.

[2] This section is not intended to be an extensive overview of Hughes's numerous contributions to sociology. Rather I have attempted to survey a representative selection of his writings in order to test their consistency with the six presuppositions. For a more comprehensive statement of Hughes's sociological position the reader may refer to Simpson's (1972:547–559) commentary on various themes in his work.

paradigm on Hughes's conception of appropriate methods of research (#2)[3] can be seen in his emphasis on the value of the comparative method for any attempt to identify the "irreducible core" of a process (Hughes, 1971:439–440). A good sociological generalization must apply to a number of social phenomena located in different settings. Thus, the various applied sociologies provide data of value in so far as they use sociological concepts and methods that are generalizable (Hughes, 1971:525). The procedures of comparative research were highly important to Hughes's studies of occupations, in which he attempted to identify processes common to all. In this work, he notes that it usually will be more enlightening to study less highly evaluated occupations, since they are less likely to disguise important processes of role definition through official symbolism (1958:49). Since his dissertation research on the institutionalization of the realtor, Hughes has demonstrated an unflagging effort to identify processes common to divergent professions.

Nonetheless, this goal did not lead to a disregard for the concrete activities of everyday life. In this respect, Hughes seems to share Park's fascination for detail. It is important to point out that Hughes, with Park, recognized movement and activity in apparently stable relations. Even in highly structured situations, much effort is required to maintain the semblance of order. In one paper, while considering whether sociology should study the "timely" or the "timeless," Hughes referred to Park's argument that each person, situation, or culture may be as revealing of general processes as another. However, in some circumstances, a unique configuration of conditions may lay open to view previously unrecognized conditions that are essential for the normal event. [Thus, the deviant case can shed light on the normal case, e.g., Lipset et al.'s (1956) study of the International Typographical Union as violating Michel's iron law of oligarchy.]

The injunction for the observer to shift

---

[3] Numbers enclosed within parentheses refer to specific presuppositions listed above.

from the specific to the general and back again, which typifies Park's practice, is adopted by Hughes as a methodological principle. Hughes traces this procedure back to Park's introduction to sociology classes, where he required students to go out into the city to observe interaction within an assigned census tract. The task was intended to allow the student to consciously observe roles while also playing the role of an observer. The value of such field work is evident to Hughes (1971:498) who notes that:

> Those of us who had a part in this program have been strengthened in our conviction that field work is not merely one among several methods of social study but is paramount.

For Hughes, to be a good observer one must be close to the people being studied. While conducting research the sociologist is always involved with the subjects, and this involvement is only apparently diminished by standardized questionnaires requiring standardized responses. Hughes traces the increased reliance of sociologists on survey research to the advancing homogeneity of language and the standardization of life in modern society. With the use of standardized instruments the eccentric is avoided and the discipline becomes methodologically ethnocentric (Hughes, 1971:476–477). Hughes's criticism does not condemn sociology to methodological indeterminancy, but simply acknowledges the possibility that new forms of collective behavior could alter present circumstances and, hence, make it necessary to caution against relying on an inflexible set of concepts (1971:467).

Two additional presuppositions of the Chicago School paradigm are reflected in Hughes's efforts to examine the intrinsic tension between institutionalization and marginality. If socialization into roles is a product of group life (#1) as the Chicago paradigm postulated, then subsequent research should examine both the institutional ordering of relationships as well as those persons located at institutional interstices. This task has occupied Hughes throughout his career. It obviously underlay his study of French Canada in Transition (1943) and many of his essays on occupations and ethnic contacts. Even

in his dissertation Hughes saw fit to emphasize the importance of institutional regulation. He observed that businessmen who made their livelihood from the sale of property, although essentially involved in an economic relationship, found it necessary to create an organization that would restrain ". . . individual initiative and freedom of action" (1931:21). If complete freedom were not possible in economic relations, which accentuated calculation, then social life, being more susceptible to non-rational action, must be even more dependent on mechanisms of social control to maintain its continuity and stability.

Of course, the consequences of group life are not all benign. For instance, Hughes locates the roots of resentment, which receive only veiled expression against authorities, but may reach their peak against those ". . . outside the sacred 'we group,'" in group pressure and cultural imperatives (Hughes, 1943:216). Despite a tendency toward ethnocentrism, groups and institutions are indispensable for the resolution of social problems. Life is ordered within the security of institutions, and in modern society much of that ordering takes place within "the world of work" (Hughes, 1971:124). The altered relationship of the person to work reflects a major structural shift in the transition from traditional to modern society, since intergenerational occupational continuity in traditional society at least allowed an individual to form a secure identity. Industrialized societies permit much greater freedom of occupational choice, and as a consequence, individuals are less certain where they ultimately will find themselves in the occupational structure. At the same time, occupations remain the most important source of identity and status. As a further consequence, to the extent the individual conforms to the value of exceeding his parents' status, the individual finds it necessary to sever relationships with peer and familial groups. The individualism of industrial culture is very difficult for the offspring of a closely knit family. They find it difficult to accept the individualism of the industrial culture since it negates the validity of much that had been valued. To live up to one's po-

tential, unfortunately, in most circumstances, means to sacrifice family expectations (Hughes, 1943:62).

Another outcome of the contemporary emphasis on individualism is that it encourages people to isolate themselves during periods of suffering and to disclaim the efficacy of ritual and social support (Hughes, 1971:128). The consequences of institutional dislocation apparent under such conditions become central to Hughes's analysis of the marginal man as a social type. According to Hughes, ". . . marginal people participate . . . only partially and on sufferance" (1943:117). Those who are marginal have not been completely accepted or assimilated into a group; they remain at the fringe of the social order and may, as in the case of the poor Southern white, threaten the stability of the system of stratification (Hughes, 1950:25). Hughes further elaborated the idea of marginal participation in social institutions in his important essay on the contradictions and dilemmas of occupational status. These contradictions and dilemmas arise from inconsistencies of unevenly ranked status characteristics, e.g., the black doctor or female airline pilot, and subsequent responses to these characteristics in different situations (1971:141).

In the wake of rapid institutional and technological change, a greater proportion of a population experiences social and geographic mobility. The accompanying accelerated change of social institutions results in the enhanced importance of consciously created voluntary associations to achieve collective goals (Hughes, 1943:122–123). This point is consistent with the Chicago School presupposition regarding the significance of conscious activity at more complex levels of social organization (#5). In combination with a more secular orientation that follows a devaluation of the past, voluntary associations designed for reaching specific goals are viewed as more likely to allow ethnic diversity. However, voluntary associations cannot assure the diffuse commitment necessary for common life that was guaranteed by the more stable and unplanned institutions of traditional society. To some extent, the potential

motivational void is being filled by the messages of radio and newspapers, which shape public opinion and mold economic and political consumption (Hughes, 1943:190). Thus, despite the persuasive appeal of the ideology of individualism, social control continues to restrain individual activity, albeit through altered forms.

The presupposition in the Chicago School paradigm that directs attention to the struggle for existence within the totality (#3) takes two analytically distinguishable forms in Hughes's work: first, in his consideration of racial and ethnic contact; second, in his studies of the influence of industrialization on more traditional social institutions. As might be anticipated from the previous comments on this presupposition, the nature of the struggle for existence ranges from unconscious ecological opposition to conscious group antagonisms. Hughes regards the division of social labor as ". . . the essence of human society" (1971:447) in that all groups have to delegate responsibilities. In this way, functions are accomplished by identifiable individuals or groups, and responsibility for common tasks is not left to chance. It should be noted that this conception of the division of labor regards the moral and technological components of life as equally important. A purely competitive system apparently will be self regulative only in the domain of biology. By contrast, a competitive economy requires some normative constraints to regulate its operation. The need to analyze both the moral and ecological spheres of social activity is explicitly expressed in Hughes's study of *French Canada in Transition* (1943), where the first two chapters include an examination of the moral context *and* population movements. Hughes considers his methodological perspective an extension of the Parkian model. That Hughes did not view either the moral or ecological order as reducible to the other, and that Park held a similar position, is emphatically asserted when Hughes rebutted a contrary claim with the statement:

If anyone is spoiling for a fight, I will gladly enter it to support the distinction between the moral aspect of society (the moral order)

and the survival aspect (the ecological) and to deny that Robert E. Park's work on cities was atheoretical" (1971:106).

The 'natural' expansion of industry into predominantly agricultural regions of the world highlights the increasing frequency of contacts between culturally distinct people. In Hughes's writings economic growth is conceived primarily as a means for adapting to the natural environment and as stimulating an equally natural population movement. These ecological processes set the context for the moral division of labor. The nature of this context is also exemplified in Hughes's study of French Canada, where he reports that with the growth of industry ". . . the tie between the town and country was loosened" (1943:92). Hence, in this setting, the solidarity of parish life was shaken by the differentiation of life styles. The best that can be hoped for in the inexorable (because it is conceived as ecological) march of industry is that an "entrepreneur" will make correct choices about material and human resources within the limits of the environment (Hughes, 1971:62).

Three further consequences of industrial expansion on ethnic solidarity can be identified in Hughes's work. First, since those who introduce industry into an area are often strangers, the subsequent changes in institutional structure contribute to minority consciousness (1943:2–3). Second, traditional values such as collective enterprise or home ownership may be eroded by wage labor and city life (1943:172). Third, the number of small merchants within the city may initially increase and threaten the status of the established merchants (1943:80). Each of these consequences implies the need for the moral structure of society (the division of social labor) to respond to the imperative of the market place.

Hughes did not simply apply, without modification, the Parkian presupposition defining the nature of the struggle for existence. For instance, he clearly distinguished between types of societies undergoing industrialization; whereas Park's analysis was more limited to the study of stages of assimilation, which, of course, could always be reversed and lead to con-

flict. The first type of industrialization specified by Hughes encompassed those societies where an indigenous population initiates industrialization and recruits labor from outside the society or from a different subculture of the same society. Park's theory of assimilation may yield valuable insights about this type of industrialization, but it is less useful in examining the emergent conditions of the second type, in which industry is brought into the locality by outsiders, who initiate only as many changes as are necessary for their purposes. Under these circumstances, social institutions are disturbed, but there is no serious pressure for assimilation. With this typology Hughes attempted to specify one condition—that is, the agent of industrial change—that affects the validity of Park's theory.

The final feature of the Chicago School paradigm I will mention in relation to Hughes's writing is the reflexive nature of social interaction (#6). This component is clearly a derivative of the pragmatist philosophic tradition as filtered through Dewey, James, Mead, and W. I. Thomas. Hughes's work manifests a strong interest in the importance of self definitions, especially as they are related to one's work. For Hughes, to change the division of labor is to change the basis of one's identity (1950:55). The labor process provides Hughes with a context for applying and extending W. I. Thomas's notion of the definition of the situation. For example, he points out that, in the professions, rules defining the situation of collegiality are extremely important since they indicate who can be trusted and under what circumstances (Hughes, 1958:47). Or, in any process of exchange, the definition of that situation signals what is considered an acceptable degree of intimacy or antagonism. Finally, Hughes notes that a definition of a situation is particularly significant in racial and ethnic contact, since the absence of a stable definition contributes to a search for a scapegoat to relieve the pressures created by institutional change.

CONCLUSION

Five presuppositions of the Chicago School's paradigm hold prominent places

in the writings of Everett Hughes. As has been illustrated above, a great deal of Hughes's research can be viewed within the scope of one or more of these presuppositions. The one presupposition identified in the Park and Burgess (1969) text that does not appear to any extent in Hughes's published work is the idea of a natural history of institutions or events.[4] Although the idea of natural history has had supporters over the years, it was one of the most controversial of the Chicago School's presuppositions. More than any of the other presuppositions identified, it presented a direct challenge to the core principle of voluntarism common to much American social thought. Perhaps that antagonism, as much as anything, suggests a reason for its negligible position in Hughes's work.

By first identifying some core presuppositions of the Chicago School paradigm and then examining the work of Everett Hughes, it has been possible to demonstrate his continuity with the initial Chicago program. It should be clear that Hughes has not seen his task as one of transmitting a pristine message from the founder. Instead, he has clarified and specified the implications of hypotheses ordered around the presuppositions. Certainly there are hints of tension between the results of some of Hughes's research and the Chicago School paradigm. On one occasion he pointed to the inaccuracy of an ideology of individualism under contemporary conditions. On other occasions he mentioned the possibility of class conflict in industrial society. And, at one point he mused about the survival of the

---

[4] In a significantly modified form, the idea of natural history does enter into Hughes's sociological perspective. His analysis of occupations and their professionalization uses the notion of natural processes of regulation and institutionalization of activity to account for the stabilizing of the division of labor in a highly complex society. This natural development is characteristic of both "proud" and "humble" occupations (Hughes, 1970:147–156). Yet I would submit that the idea of natural history as formulated in Park and Burgess's *Introduction* referred to a structural process applicable to the analysis of growth at the community or societal level. It implied a quasi-evolutionary theory of change that does not appear in Hughes's work, or for that matter in the later writings of Park.

nation-state with the growing difficulty of maintaining a colonial empire. But these themes are never developed to a point where they would offer a structural theory of conflict that would counter the naturalistic individualism of the Chicago paradigm.

The conjunction between the Chicago School paradigm and the sociological writing of Everett Hughes is borne out by this discussion. However, further study of other members of the Chicago School who had varying degrees of interaction with Park and/or Burgess at different times in their careers is necessary to more fully appreciate the amount of impact that an academic community and a set of shared presuppositions have on a scholar's work. This paper has only outlined some of the themes apparent in the core theoretical/ methodological text of the Chicago School, and has indicated manifest congruences in the work of Everett Hughes. Additional specification of these presuppositions might be accomplished by examining other publications of the founder-leaders through the course of their careers. Formal and informal relationships between the members of this school need to be made more explicit, to explain, perhaps, differing commitment to the identified presuppositions. Finally, these same tasks need to be undertaken with respect to the "schools" that Tiryakian (1979:223–231) suggests were formed around Durkheim and Parsons. When these projects have been accomplished, it will be possible to assess more completely the relative importance of schools in shaping the discipline of sociology.

REFERENCES

Braude, Lee
   1970   " 'Park and Burgess:' An appreciation." American Journal of Sociology 76:1–10.
Carey, James T.
   1975   Sociology and Public Affairs. Beverly Hills, CA: Sage Publications.
Coser, Lewis A.
   1971   Masters of Sociological Thought. New York: Harcourt Brace Jovanovich.
Faris, Robert E. L.
   1967   Chicago Sociology, 1920–1932. Chicago: University of Chicago Press.

Hughes, Everett C.
   1928   "Personality types and the division of labor." American Journal of Sociology 23:754–768.
   1931   The Growth of an Institution: The Chicago Real Estate Board. Chicago: The Society for Social Research, University of Chicago.
   1943   French Canada in Transition. Chicago: University of Chicago Press.
   1950   "Race and Culture Contacts." Lecture Notes. Durham, NC: Duke University Library.
   1958   Men and their Work. Glencoe, IL: The Free Press.
   1970   "Humble and the proud: The comparative study of occupations." Sociological Quarterly 11:147–156.
   1971   The Sociological Eye. New York: Aldine-Atherton.
Hughes, Everett C. and Helen MacGill Hughes
   1952   Where Peoples Meet. Glencoe, IL: The Free Press.
Janowitz, Morris
   1978   The Last Half-Century: Societal Change and Politics in America. Chicago: University of Chicago Press.
Lipset, Seymour, Martin Trow, and James S. Coleman
   1956   Union Democracy. Garden City, NY: Doubleday.
MacPherson, C. B.
   1962   The Political Theory of Possessive Individualism. New York: Oxford University Press.
Martindale, Don
   1960   Nature and Types of Sociological Theories. Boston: Houghton Mifflin.
Matthews, Fred H.
   1977   Quest for an American Sociology. Montreal: McGill-Queen's University Press.
Nisbet, Robert
   1966   The Sociological Tradition. New York: Basic Books.
Park, Robert E.
   1915   "The city: Suggestions for the investigation of human behavior in the city environment." American Journal of Sociology 20:577–612.
Park, Robert E. and Ernest W. Burgess
   1969   Introduction to the Science of Sociology.
   (1921)   Chicago: University of Chicago Press.
Park, Robert E., Ernest W. Burgess, and Roderick D. McKenzie
   1967   The City. Chicago: University of Chicago
   (1925)   Press.
Raushenbush, Winifred
   1979   Robert E. Park: Biography of a Sociologist. Durham, NC: Duke University Press.
Rucker, Darnell
   1969   The Chicago Pragmatists. Minneapolis: University of Minnesota Press.
Simpson, Ida Harper
   1972   "Continuities in the sociology of Everett C. Hughes." The Sociological Quarterly 13:547–559.

Tiryakian, Edward A.
    1979 "The significance of schools in the devel-
          opment of sociology." Pp. 211–233 in
          William E. Snizek, Michael K. Miller, and
          Ellsworth R. Fuhrman (eds.), Contempo-
          rary Issues in Theory and Research: A

Metasociological Perspective. Westport,
CT: Greenwood Press.

Turner, Jonathan H.
    1974 The Structure of Sociological Theory.
          Homewood, IL: Dorsey Press.

THE STRUCTURE OF THE CONTEMPORARY SOCIOLOGICAL PROBLEMATIC: A FOUCAULTIAN VIEW*

BENG-HUAT CHUA

Trent University

This essay argues that some of the central theoretical issues debated from different perspectives within sociology should not be conceived as disparate, independent, and diachronic, even if competing, unfoldings of different paradigms. Using Foucault's substantive discoveries of the epistemological rupture in Western thought, it argues that these theoretical formulations may be conceived as a synchronic set of questions that results from (1) the emergence of language as an object in its own right, and (2) Kant's investigation of the problem of representation at the end of the eighteenth century. It argues that the meeting between these two events frames the four related issues that constitute the contemporary sociological problematic. The issues are: (1) the materiality and mediation of language in knowledge production; (2) the analytic status of subjective consciousness in knowledge claims; (3) the hermeneutic problem of the "theoretical unconscious" in both theories and theorists; and (4) the problem of the "unthought" if all truths are consensually language-based.

A history of sociological theory can be written from a sociology of knowledge perspective: the social political position occupied by the theorist analytically may be said to crystallize in the theorist as a moralistic disposition and a concomitant political consciousness. Furthermore, these may be analytically said to be embedded in the theory that is produced (Gouldner, 1970). Alternatively, it may be argued that the historical social political condition is itself crystallized into a prevailing political sentiment. Analytically, this political sentiment may be attributed to a contemporary theorist: and that he or she comes to pose theoretical questions in ways informed by this political sentiment (Nisbet, 1967). Finally, this social political condition may be analytically used as the negative background against which theorists struggle to formulate their ideas (Axelrod, 1979).

This history can also be written in terms

of conventional history of ideas: a theoretical formulation and its theorist are analytically located at a stage in the continuous, progressive unfolding of an original set of ideas, initiated by an original thinker. This original set of ideas is analytically treated as constituting the conceptual boundaries of a tradition of which a particular theoretical formulation in question is a part. All of our short hand versions of sub-fields, and contributions to sub-fields of sociology such as structural-functionalism, symbolic interactionism, and Marxism embody such a conception of tradition. Or a set of ideas may be set up as a counter tradition, without altering its analytical position or import, against which a theoretical formulation may be interpreted and evaluated, positively or negatively; for example, the case of arguing that Durkheim and Weber were struggling against the theoretical formulations of Marx (Zeitlin, 1969).

These approaches tell us little about how, at any one time, the various features of the sociological discourse come to be synchronically present, and why they

constitute a conceptual problematic within which sociological questions not only are posed but from which they derive their rationality as reasonable sociological questions.[1] There are two reasons. First, because conventional sociology of knowledge tends to emphasize the social political location of the knower as the external causal determinant of the knowledge that is produced, it also tends to be a sociology of knowledge in which the systematization of knowledge and its production at specific historical periods are deleted. At its best it can account for the systematization and continuity of a particular theorist's work, but never the systematization of the conceptual conditions in which the theorist works and within which a theoretical formulation finds its rational foundation. The question of systematization of knowledge and its production, independent of specific theorists, is left with the epistemologists. This division between epistemology and sociology of knowledge is progressively shown (Chua, 1978) to be untenable, for epistemological questions are often tied to sociological and political dimensions of knowledge production. This insight provides for the possibility and necessity of sociology of knowledge in the first place (Wolff, 1959:571–572).

Second, while the continuous history of ideas perspective tells us much about the diachronic developments of particular sociological traditions, it does not address the synchronic relations of the different perspectives in the discipline. Since sociology is always constituted of ostensibly competing frameworks of analysis, this neglect is a significant problem.

Tracing the internal developments of individual traditions, or in the case of sociology of knowledge, the internal development of a particular theorist, neither characterizes nor produces an adequate description of this situation of multiple frameworks. The convenience of characterizing, and contented acceptance of, sociology as a multiparadigm science (Ritzer, 1975; Effrat, 1972; Overington, 1979) only avoids the issues of explicating the rationality of the synchronic presence of the multiple perspectives themselves.

These problems point to a need to investigate the synchronic epistemic field in which sociology is located. The questions that are posed from different perspectives within contemporary sociology as a discipline may have their rationality already provided for by the epistemic field. If so, resolution of epistemological issues must be effected by shifts in the epistemic field itself rather than in the interstices of the middle grounds between the mutually exclusive and competing theoretical frameworks.

Two projects are implied. One is to elaborate an approach for the suggested investigations. The other is the substantive task to situate the contemporary sociological problematic as a conceptual unit within the contemporary epistemic field. The writings of Foucault are appropriate for both projects. This essay, however, covers only the second project.

The essay will draw upon Foucault's (1970) substantive investigation of the epistemological changes in Western thought at the beginning of the nineteenth century. According to Foucault, two important discursive events took place; namely the emergence of language as an object in its own right and Kant's two-sided investigation of the problem of representation.[2] This essay will demon-

---

[1] The term synchrony is used throughout this essay to signify not only the historical copresence of the different conceptual features in a discipline, but also the relationships that exist between these features; the relative coherence of these relationships as a conceptual system is what we call a conceptual problematic. Even though this coherence is only relative, i.e., historically temporary, it must be emphasized. It is this coherence that provides us ground to conceive history, including history of discourse, discontinuously, for changes in a problematic involve not only changes in individual conceptual features but also the entire problematic. For a brief discussion of Foucault's conception of discontinuity and transformation in discourse see footnote 2 in this essay.

[2] For Foucault, all statements are events in discourse in the most materialist sense, because as a statement it is first "linked to the gesture of writing or to the articulation of speech . . . it opens up to itself a residual existence in the field of a memory, or in the materiality of manuscripts, books or any other form of recording; secondly, because, like every event, it is unique yet subject to repetition, transformation, and reactivation; thirdly, because it is linked not only to the situations that provoke it, and to the

strate that the central features of the contemporary sociological problematic share the same epistemological site and point of emergence because they may be situated within the epistemological spaces opened up by the meeting of these two events, and that this commonality provides for their synchrony as a problematic. A summary of Foucault's characterization of the emergence of these two events is, therefore, immediately in order, following which the essay will show how these two features undergird the theoretical issues currently debated.

EPISTEMOLOGICAL RUPTURE AT THE NINETEENTH CENTURY

According to Foucault, during the seventeenth and eighteenth centuries, the study of knowledge was concerned with the search for a universal science of order based on systems of signs. The systems of signs were always conventional, even if a sign was a natural sign such as breathing denotes life. As a sign breathing was "no more than an element selected from the world of things and constituted as a sign by our knowledge" (Foucault, 1970:61); natural signs therefore were rudimentary versions of conventional signs. This conventionalism did not give rise to the problems of relativism which now plague us, because it was forestalled by the function that signs were to play in the analysis of order:

> In its perfect state, the system of signs is that simple, absolutely transparent language which is capable of naming what is elementary . . . (Foucault, 1970:62).

An idea or an object was considered a sign, a representation, of another object only when the representing object was itself emptied of content, of function, and in no way determined what was represented; its content was none other than that of the object represented. That was why the Port Royal grammarians of the period used pictures and maps rather than words or symbols as the paradigmatic example of

consequences it gives rise to, but at the same time, and according to quite different modality, to the statements that precede and follow it" (1972:28; see also Lemert, 1979a:9).

signs; "the picture has no other content in fact than that which it represented, and yet the content is made visible only because it is represented by a representation" (Foucault, 1970:60). There was therefore no separation between a theory of signs and analysis of meanings; the meaning of a sign was exactly the meaning of the object represented. At the end of the eighteenth century, two discursive events shattered this transparency.

Within the epistemological context of the eighteenth century, the study of language was concerned with the analysis of the representative values of the word. One strategy for cross language comparisons was to hold a root constant; the series of forms that the root took in each language could then be compared. This strategy was grounded in the assumption of an original and common language which supposedly provided the initial group of roots to all languages (Foucault, 1970:234). At the close of the eighteenth century, when the series of forms of a root in one language was compared with a corresponding series of another language, it was found that the inflection systems of the two series were analogues while the forms were modified. What was being discovered was the grammatical function of the inflectional system in the modification of meanings. This tiny discovery opened up the entire universe of grammatical relations internal to language.

From the nineteenth century on, language was no longer devoid of form and content, and no longer served the function of representation completely. It emerged as a system of formal elements that acted on the sounds, syllables, roots, and words, and imparted to the latter an organization independent of representation; instead it determined the possibility of representation (Foucault, 1970:280). The grammatical rules not only operate outside of representation but also turn language in on itself by defining for language an interior space and density that are constitutive of language as an object.

This objectification of language was historically synchronous with Kant's two pronged investigation of the very possibility of representation itself. On the one hand, representation was questioned

from the point of what makes representation possible. According to Foucault (1970:250–302), this led to the positing of the transcendental subject, which possessed *a priori* all the necessary formal conditions of experiencing and determining its relation to the object. On the other hand, the question was reversed and posed from the point of the object represented. In this case, the objects were posited with internal spaces and densities as the foundation of the external features that were given to human experiences. The objectification of language was itself an instance of this. These internal densities turned the realities and essences of objects into themselves; they constituted the domains of empirical *a posteriori* knowledge that knowledge must attempt to uncover. ·Representation was thus detached from its identity with objects represented and from its identity with knowledge; it became the external relation that the objects bear with human experience which as human consciousness has the power to present objects to itself (Foucault, 1970:313).

Parenthetically, Foucault does not account for these emergences as systematic consequences or effects of some identifiable specific causes; i.e., he does not account for this change as a continuous unfolding of some earlier events. Indeed, while every one of his substantive institutional studies (1965; 1973; 1979) deals with changes in the technical, political, and discursive practices in these institutions, the changes are always displayed through side by side presentation of synchronic descriptions of each of the different stages; i.e., the history of the institution in question is presented discontinuously, and it is through these discontinuities that changes or transformations are made observable. This presentation has been criticized as a failure to account systematically for the changes (Foucault, 1970:xii). Within French structuralism, where discontinuity in itself is not only not raised as an issue but rather is theoretically preferred, this supposed failure is conceived as a failure to provide a system of rules of transformation for the analysis of discourse (Piaget, 1970:128–135). In his own defense, Foucault has vehemently

argued against the rationality of continuous history (1972:3–14). Furthermore, he argues that transformations in discourse are linked to a whole range of usually complex modifications that can occur outside of the domain of discourse, inside it, or to the side of it in other discourses, and that "[T]hese transformations cannot be reduced to precise and individual discoveries; and yet we cannot characterize them as a general change of mentality, collective attitudes, or a state of mind" (1977b:200). Consequently, Foucault is a methodological pluralist on this point (1978). From his position, to articulate a general theoretical conception for changes in discourse would amount to prejudging events by theoretical fiat. For his part, Foucault has been relying on historical events themselves to display the transformations, and is theoretically willing to accept chance as a component of changes (1971:24). The issues, nonetheless, are not settled, and require further clarifications that are beyond the substantive interest of this essay.

In any case, after Kant, knowledge is always anterior to language, either from the point of the transcendental subjective consciousness which possesses all the formal conditions for knowledge, or from the side of the object that awaits to be known. But knowledge must express itself in language; consequently, it is dependent upon the possibilities permitted by the rules of grammar. This mediation of language is never a satisfactory situation, for the representation of knowledge by and through language is never without residue (as was the case until the late eighteenth century) on the side of knowledge, either in terms of subjective consciousness or the objective existence of things.

As a subjective consciousness, a knowing subjectivity, a human being is capable of investigating language itself as a finite object of his knowledge. On the other hand, this subjective self-consciousness inevitably recognizes that language is historically prior to itself, that the entire historical solidity of language is at once and endlessly activated by the very first expression of the subjective consciousness. This subjective consciousness is trapped, without liberation,

within the inner historical density of language; one becomes a vehicle of words that are anterior to oneself. In this double relation between subjective consciousness and language as an object, man is both a subject and an object of his own language; he is both the vehicle of language and the initiator of speech.

The contemporary sociological problematic can be situated within the epistemological spaces opened up by the meeting of language as an object and the double-edged Kantian inquiry of the basis of representation. At this meeting point, four sets of problems can be identified: (1) the mediation of language in knowledge; (2) the analytic status of subjective consciousness; (3) beneath language and consciousness, the hermeneutic problem of theoretical unconsciousness, and (4) beyond language and contemporary consciousness, the problem of the "unthought." It will be demonstrated that these problems constitute the contemporary sociological problematic, and as such determine the ways questions are posed in contemporary sociological theorizing. In all of these questions the central assumption is, as Habermas puts it, that "language not only bounds itself off from external or objectified nature, against the normative reality of society, and against subjective nature, but also, as it were, opens itself osmotically to them" (1976:155).

MEDIATION OF LANGUAGE

As a result of sustained self-reflexive criticism of positivistic sociology since the late sixties, sociologists have come to recognize the materiality of language as a mediation to all claims of knowledge. They recognize that language as a conventional system of signs—an object in its own right—partakes unavoidably in constituting the meaning and significance of things it names. To the extent that much of the achievement in sociology so far is built upon the "transparency" of language, this recognition inevitably produces a renewed critique of the epistemological grounds of sociological theorizing (Chua, 1978). The rise of various neo-interpretive sociologies as the

alternatives to structural functionalism and operationalism attests to these developments. As the following discussion will show, the central epistemological issue introduced by the insistence that knowledge is language-mediated truth is the problem of relativism.[3] This is already well known in anthropology as the problem that surrounds the Sapir-Whorf Hypothesis (Langacker, 1976).

While early ethnomethodologists, for example, see themselves as "doing description" of members' methods of producing social reality, this descriptive claim remains problematic. Central to ethnomethodology is the conception that social activities partake in the constitution of the reality of objects as observable and factual; that this is necessarily so provides the ethnomethodologists with their objects of inquiry. The central social activities are linguistic activities. Through language, members show, tell, and accomplish for each other what is to be observed, and how it is to be observed within the social situation. Each of these instances (Chua, 1979; Wieder, 1974; Silverman, 1974; Smith, 1974) is itself a result of members' methodic practices grounded in a conventionalized way of knowing the world. This stance draws its support from the constitutive feature of language. It is in part why ethnomethodologists are interested in late Wittgensteinian theory of language (Coulter, 1973). This lends support, although not entirely convincing, against charges of subjectivism levelled at ethnomethodology (Maryl, 1973; Gellner, 1975; Coser, 1975; McSweeny, 1973). While ethnomethodology may avoid subjectivism at some levels, it is nevertheless continuously plagued by relativism in

---

[3] This section on mediation of language is summarized from Chua (1978). In a recent publication that I have not had the chance to incorporate into this essay, Joachim Israel suggests that in an attempt to develop a Marxist, i.e., materialist, methodology for social psychology, he was immediately confronted with the problems of the mediation of language in knowledge production. He argues that "the sharp dualistic distinction between knowledge of language, and knowledge of reality, has to be abandoned," for "knowledge of language is knowledge of reality" (1979:xiv).

other ways. The discussion on metaphor will clarify this.

Perhaps the sociological works that are most explicitly indebted to the constitutive philosophy of language are those that turn on the nature of linguistic metaphors. The conception of metaphor currently favored by philosophers stresses the cognitive rather than descriptive function of metaphors; i.e., the application of a metaphor enables the user to "see" characteristics pertaining to the thing rather than describing what the thing is. Metaphors reformulate things and bring forth features constitutive of these things (Edge, 1974; Ricouer, 1974). The history of sociology may be viewed as filled with such cognitive metaphors; each ebbs and wanes in different periods, such as "society is an organism," of Durkheimian, Spencerian, or Parsonian versions, or "society is a theatre." From the theoretical perspective that stresses the constitutive character of metaphors, none of the results arrived at by any of these metaphoric renderings of society can lay descriptive claims in the empiricist sense. What results there are, however useful, must be conceived as achieved or achievable by the imposition of the metaphors themselves and not descriptive of the given ontological features of society. The consequence is, as one major proponent of reading sociological theories as metaphors puts it, "even if we recognized that models (metaphors) cannot present the world as it 'really is,' however, it still might be argued that the 'really is' world is nonetheless obdurately there;" epistemologically we will have a compromise, "a relativism of models alongside an absolutism of phenomenon" (Brown, 1976:184).

To the extent that ethnomethodology assumes a constitutive function for language, then it cannot avoid this relativism. If language is constitutive of the reality of the object, then language difference necessarily produces different realities. In this contention, differences in cultures are not even a necessary condition to raise the problem of relativism. From Bernstein's (1971) work on the mastering of expanded and restricted linguistic codes among members, questions regarding the nature of *the* social reality may be raised within the same society. Within ethnomethodology, however, this relativism among members tends to be sidestepped by the assertion of "reality" as intersubjective. The assumption here is, as Pollner puts it, "the primordial suppositions which infuse our conception of our relations to the world and which posit a commonly or intersubjectively shared order of events continues to be employed by most people" (1974:35).

PROBLEM OF SUBJECTIVE CONSCIOUSNESS

These same sociologies also emphasize the role of the subjective consciousness in the production and consumption of knowledge, i.e., the other side of the double relations between language and subjective consciousness. Symbolic interactionists and ethnomethodologists both emphasized the "creative" activities of the situated actors in bringing off an observable and accountable objective social reality (Morris, 1975; Hewitt and Hall, 1973; Heap, 1976). However, given that our concerns as sociologists are social reality constructions and not private worlds, this emphasis on subjective consciousness can be maintained analytically only with great difficulty. In the analysis of the social, the accent is always on what is publicly observable.[4]

In emphasizing subjective consciousness, language is conceived as a representation of this consciousness with the proviso that language may fail to fulfill this representative function. Significantly, the primacy and anteriority of subjective consciousness is both more pronounced and more eclipsed when there is disagreement among interactants: more pronounced because disagreements point to the failure of language to represent the interactant's thoughts which activate language in the first place; more eclipsed because, at the point where the identity of language and subjective thoughts is broken, language emerges as an object with its own density, and thoughts seeking to express them-

---

[4] In the analysis of the social we are only interested in the attention paid by "Anybody Upon Anybody" (Jefferson, 1973:95).

selves are immediately trapped and subju-
gated to the multiple possibilities of lan-
guage. This gives rise to possible mis-
representations and misapprehensions
and hence disagreements. The sociologi-
cal interest is one of finding the grounds
which render agreements among interac-
tants possible.

The result is that, contrary to the initial
affirmation of subjective consciousness,
there appears to be an elevation of the
materiality of language over subjective
intention, for it is argued that the agree-
ments are generated by the correct usage
of language, according to the semantic,
syntactic, and contextual rules that are
constitutive of language as an object. It is
in this sense that the concept of grammar
of social interaction (Blum and McHugh,
1971) or grammar of accounts (Hewitt and
Hall, 1973; Scott and Lyman, 1968) is in-
telligible. The elevation of language is also
evident in the analysis of the determining
effect of stylistics on the production of
sociological knowledge (Gusfield, 1976;
Lofland, 1974).

HERMENEUTICS OF THEORETICAL
UNCONSCIOUS

While the grammatical features of lan-
guage point to the constraints of what can
be expressed, its philological density points
to what is not expressed.

> Having become a dense and consistent
> historical reality, language forms the locus of
> traditions, of the unspoken habits of thought,
> of what lies hidden in a people's mind, it
> accumulates an ineluctable memory which
> does not even know itself as memory. Ex-
> pressing their thoughts in words of which
> they are not the masters, enclosing them in
> verbal forms whose historical dimensions
> they are unaware of, men believe that their
> speech is their servant and do not realize that
> they are submitting themselves to its de-
> mands (Foucault, 1970:297).

From this assumption emerges "the need
to work one's way back from opinions,
philosophies, and perhaps even from sci-
ences, to the words that made them possi-
ble, and beyond that, to a thought whose
essential life has not yet been caught in the
network of any grammar" (Foucault,
1970:297–298). This is the epistemological

ground for our sociology of knowledge
and sociology of sociology as they cur-
rently are practised.

Typical of sociology of knowledge and
sociology of sociology, the methodolog-
ical strategy is a hermeneutics that mis-
trusts what is expressed in a discussion, a
speech, or an essay; a strategy aimed at
disturbing the surface unity of the dis-
course and dispersing its statements so as
to uncover the thoughts and influences
that loom behind the speech. An example
of this is Gouldner's conception of the
object and objectives of Reflexive Sociol-
ogy:

> Reflexive sociology attributes importance to
> the theorist's infrastructure—his domain as-
> sumptions, his sentiments, the things that
> are real to him, and the ways these things
> shape his theory. It emphasizes the manner
> in which such assumptions affect his per-
> ceptions of the larger society and provide the
> human grounding out of which social theory
> grows (Gouldner, 1973:78).

What are uncovered are variously known
in sociological discourse as the deep
structures, the commitments, the forms of
life, the domain assumptions, and the
ideologies,[5] or, generically, the "theoreti-
cal unconscious" of the speaker.

The result is often a commentary which
claims to say for the first time that which
is already silently articulated, but never-
theless remains hidden and which sup-
ports from this hidden position the dis-
course in question (Foucault, 1971:13). As
commentary, it must in turn serve as a
primary text for a second commentary and
so multiplies indefinitely. For example,
once on to the hermeneutic exercise of
explicating the unsaid of other's writings,
McHugh et al. (1974) find themselves in
need of each other to explicate each
other's grounds for their speech. But this
being an interminable exercise, this col-
laboration in explication is extended to the
readers/hearers of their speech. As Gid-

---

[5] Habermas (1970) conceptualizes ideology as a
distorted communication that requires a
psychoanalytic hermeneutics to cut a path to the
possibility of real political consensus characterized
by uncluttered speeches and debates. Another
example of hermeneutics of unconscious is Jones's
(1977) argument for uncovering the intents of
theorists as part of the history of sociology.

dens puts it: ". . . intrepid travellers all, now left swirling helplessly in the vortex of the hermeneutic whirlpool" (1976:166).

The various formulations and explications of the theoretical unconscious of a speech as a shortcoming of both theories and theorists in sociology serve as the ground for their alternative: a prescription to bring to consciousness, to display and to proclaim these hidden elements in the theories themselves. Here again, Gouldner's formulation of Reflexive Sociology as radical sociology serves as an elegant illustration:

> Radical, because it would recognize the knowledge of the world cannot be advanced apart from the sociologist's knowledge of himself and his position in the social world, or apart from his efforts to change those. Radical, because it seeks to transform as well as to know the alien world outside the sociologist as well as the alien world inside him. Radical, because it would accept the fact that the roots of sociology pass through the sociologist as a total man, and that the question he must confront, therefore, is not merely how to work but how to live (Gouldner, 1970:489).

It should be apparent that there is an emphasis on the analytic significance of the subjective consciousness of the theorist.[6] Yet no sooner is the significance of subjective consciousness asserted, than the primacy of language raises itself again in Gouldner's very own formulations, as the next section will show.

THE UNTHOUGHT

At the meeting point between the grammatical and philological dimensions of language is the site of yet another manifestation of the contemporary sociological problematic. If all knowledge is mediated by the density of language then what passes as truth has no authority outside a specific discourse. (Recall here the discussion of relativism earlier in this essay.) Consequently, if a historically and language-specific truth is to be translated and imposed as a universal statement (Truth), this move constitutes either an

illusion or a political willfulness, an authoritarianism. To avoid either or both consequences, a theorist is forced to think of the unthought, that which is not yet but may nonetheless potentially modify existing truths. This unthought resides in that ever receding horizon, for each time thought moves towards it, the unthought is captured again and formulated according to language. In practice this unthought can be preserved only as an ethos, one which Gouldner formulates as both the demand for "new languages" and the ability to receive "bad news." Both are constitutive of a morality of objectivity (1976a; 1976b).[7]

If all knowledge is language mediated, where is the source for "new languages," and what are their functions? The source of new language lies in the subjective consciousness' ability to formulate and bring forth characteristics of objects as they are represented and made manifest through language; the function of the new languages is to force the community of speakers to adjust themselves to each other's speeches and ultimately adjust to the posited objectivity of things.

"Bad news" signifies the possibility of a community's claim to be in error, but if all truths are consensually achieved, language constituted claims, including of course the endless possibility of adjusting to new languages, where is the source of error? The possibility of error must be grounded in the objectivity of things; while never reached by language it nonetheless looms in the horizon of our language and knowledge—hence the multiplicity of metaphors and absolutism of objects, and the interminable possibility of new languages and bad news.

---

[6] For details of the emphasis of subjective consciousness as a negative feature in contemporary sociologies see Lemert and Gillian (1977).

[7] Habermas, like Gouldner, also maintains that truth and knowledge are always language-mediated, and they both maintain a consensus theory of truth. Gouldner's conception of the morality of objectivity parallels Habermas' desire to establish "the ideal speech situation" in that both are strategies aimed at preventing a consensual truth from sliding into political and epistemological authoritarianism; they both are attempts to preserve the possibility of rational agreements in discourse. For a discussion of Habermas' position see McCarthy (1973). Indeed, the entire Habermas' corpus may be shown to cut across all the four features of the sociological problematic; see Lemert (1979a:194–225).

In this double affirmation of the constitutive operations of both language and the subjective consciousness in knowledge production is the persistent problem of relativism, which is, of course, derived from the problem of objectivity. Within the contemporary epistemological conception, the problem with relativism is that it undermines its own claims to knowledge, for it has no authority beyond its own premise. This above all else, Gidden argues, is relativism's most telling problem as a serious epistemological stance (1976:145). At this point, the positivist program raises itself and persists as a credible theoretical postion; that is, of course, the other side of Kant's inquiry of representation.

From the side of the objects represented by language, the possibility of knowledge lies in the conditions of the possibility of the objects' existence. The contaminations of language and subjective consciousness must be purged. This is why positivists demand either a neutral language which "could become the exact reflection, the perfect double, the unmisted mirror of a non-verbal knowledge" (Foucault, 1970:296) or the development of a formal language. The project is essentially one of searching "for a logic independent of grammars, vocabularies, synthetic forms and words: a logic that could clarify and utilize the universal implications of thought" (Foucault, 1970:296), without being contaminated by natural language. Here we can locate the tendencies toward formal mathematic sociological theories which act as a counterpoint to language based sociology. The latter sees the entire positivist's project as a dream or a blindness (McHugh, 1970).

It now should be apparent that the "two sociologies" (Coser, 1975) emerged historically with the epistemological ruptures of the late eighteenth and early nineteenth centuries. Each requires the other to formulate its own programme, and the two form two divergent but symmetrical developments and advances in the production of sociology as a discipline. Attempts, couched almost always in some "dialectical" terms, to integrate these diverging movements remain unconvincing. For in most of these formulations, there is in-variably a double affirmation of both sides of the Kantian question without being able ever to specify the limits, the beginnings, and ends of when one affirmation gives way to the other. The result is a discourse that *ad hocs* along with the double affirmations and, unavoidably, double negation in its unfolding.

As sociologists' understanding of language continues to expand, and as this understanding continues to influence their formulations, the balance between the two sociologies seems to be slipping in favor of the language centred sociologies, at least at the theoretical level, because even the most committed positivists cannot deny the presence of language in their formulations. In this context, and drawing from a combination of the writings of various French structuralists with a large dosage of Foucault's *The Order of Things* (1970) and *The Archaeology of Knowledge* (1972), Lemert (1979a; 1979b) is making an important attempt to transport us to the full light of an entirely language based sociology. The issues to be raised in his attempt must be focused on the role of the subject as both the initiator of speech and the vehicle of language (Hymes, 1979). This is, of course, another topic. Suffice it to suggest here that it is precisely because human beings are initiators of their own speeches that the tension between subjective consciousness and the materiality of language continues to occupy the center of sociological discourse. Furthermore, while Lemert, following Foucault's (1970) suggestion of the eclipse of man in the twentieth century episteme, is intent on purging the subject from sociological discourse, he is at least morally, i.e., politically, ambivalent about this move: "One cannot be sure whether to cry or rejoice at the death of man" (Lemert, 1979a:xii).[8]

─────────

[8] This essay was developed independently of Lemert's work (1979a; 1979b). Professor Lemert also locates the contemporary sociological theories within Foucault's discovery of the epistemological break of the early nineteenth century. In his desire for a language-based sociology, Lemert has stressed the linguistic concerns of the French thinkers that he draws upon (1979a:10). In turn, he neglects these thinkers' concern with questions of consciousness, such as Levi-Strauss' anthropology of the mind, Derrida's phenomenology (1978), and Foucault's concern for individual liberation, which can be

CONCLUSION

The synchronic presence of different theoretical stances and methodological strategies in sociology at any particular historical period is not just a chance meeting of the different traditions unfolding according to each one's own trajectory. Nor is the history of sociology, as a discipline, that of one dominant theoretical stance succeeding another after the first one is bogged down with theoretical, conceptual, and methodological difficulties. While sociologists may not always recognize the first point, they are well aware of the second in that they accept rather readily the idea that sociology is a multiparadigm science. Neither the conventional history of ideas approach nor the sociology of knowledge approach attempts to account for the very copresence of these perspectives over time.

With the delineation of some of the central issues debated from different angles and by different perspectives, rather than looking at the internal development of each perspective, this essay demonstrates that the different poses are divergent developments that emerge from the same epistemological site produced by the discontinuous changes in the conception of the function of language and of representation at the close of the eighteenth century. Sharing this same epistemological ground, the divergent strategies are mutually defining and mutually elaborating. Consequently, the resolution of the problems that appear at the meetings of the various positions may lie not in the synthesis of these positions, because they gain their grounds of copresence precisely in their differences. The resolutions may require a radical shift of the epistemological bedrock which, while perhaps desired by sociologists, may act anachronistically independent of sociologists, for such shifts could emerge as tiny events in any discipline.

Finally, while this essay draws from Foucault's substantive discoveries in the

discursive formations of the human sciences (1970), it must be noted that his interest is not in these discoveries themselves, however useful they may be. Foucault's interest is not limited to criticizing the human sciences' continued captivity within the nineteenth century's concern with the human subject that seems to bog us down in whirlpools of hermeneutic circles and endless search of the every receding origin. Rather, he places his own studies of discursive formations and discursive practices in the institutional exercise of power as exemplars of a way forward, a way out of the whirlpools. In his writings may be found an approach, already loosely formulated as method (Foucault, 1972), which will redirect sociological investigations beyond using it for analysis of sociological theories themselves.

REFERENCES

Axelrod, Charles D.
  1979 Studies in Intellectual Breakthrough: Freud, Simmel and Buber. Amherst: University of Massachusetts Press.
Bernstein, Basil
  1971 Class, Codes and Control. London: Routledge and Kegan Paul.
Blum, Alan and Peter McHugh
  1971 "The social ascription of motives." American Sociological Review 35:98–100.
Brown, Richard
  1976 "Social theory as metaphor." Theory and Society 3:169–198.
Chua, Beng-Huat
  1979 "Describing a national crisis: An exploration in textual analysis." Human Studies 2:47–61.
  1978 "Sociology and the linguistic distance." Philosophy and Social Criticism 5:174–190.
Coser, Lewis
  1975 "Two methods in search of a substance." American Sociological Review 40:691–700.
Coulter, Jeff
  1973 "Language and the conception of meaning." Sociology 7:173–189.
Derrida, Jacques
  1978 " 'Genesis and structure' and phenomenology." Pp. 154–168 in Writing and Difference. Alan Bass (trans.). Chicago: University of Chicago Press.
Edge, David
  1974 "Technological metaphor and social control." New Literary History 6:135–148.
Effrat, Andrew
  1972 "Power to the paradigms: An editorial introduction." Pp. 3–35 in Andrew Effrat (ed.), Perspectives in Political Sociology. Indianapolis: Bobbs-Merrill.

---

gleaned from his preface to Deleuze and Guattari's *Anti-Oedipus* (1977a:xiii-xiv). Contrary to Lemert, my concern is to describe and perhaps preserve the tention between language and subjective consciousness in sociological discourse.

Foucault, Michel
1979 Discipline and Punish: The Birth of the Prison. New York: Vintage Books.
1978 "Politics and the study of discourse." Ideology and Consciousness 3:7–26.
1977a "Preface." Pp. i-xxiv in Gilles Deleuze and Felix Guattari, Anti-Oedipus. Robert Hurley, Mark Seem, and Helen Lane (trans.). New York: Viking.
1977b "History of systems of thought." Pp. 199–204 in D. F. Bouchard (ed.), M. Foucault: Language, Counter-Memory and Practice. Ithaca, NY: Cornell University Press.
1977c "A preface to transgression." Pp. 29–52 in D. F. Bouchard (ed.), M. Foucault: Language, Counter-Memory and Practice. Ithaca, NY: Cornell University Press
1973 The Birth of the Clinic. London: Tavistock.
1972 The Archaeology of Knowledge. London: Tavistock.
1971 "Orders of discourse." Social Science Information 10:7–30.
1970 The Order of Things. New York: Vintage Books.
1965 Madness and Civilization. New York: Pantheon.
Gellner, Ernest
1975 "Ethnomethodology: The re-enactment industry or the California way of subjectivity." Philosophy of Social Sciences 5:431–450.
Giddens, Anthony
1976 New Rules of Sociological Method. New York: Basic Books.
Gouldner, Alvin
1976a "The dark side of the dialectic." Sociological Inquiry 46:3–15.
1976b The Dialectics of Ideology and Technology. New York: Seabury Press.
1973 For Sociology. New York: Basic Books.
1970 The Coming Crisis of Western Sociology. New York: Basic Books.
Gusfield, Joseph
1976 "The literary rhetoric of science: Comedy and pathos in drinking research." American Sociological Review 41:16–34.
Habermas, Jurgen
1976 "Some distinctions in universal pragmatics." Theory and Society 3:155–167.
1970 "Towards a theory of communicative competence." Pp. 114–118 in Hans Peter Dreitzel (ed.), Recent Sociology 2: Patterns of Communicative Behaviour. London: Collier-Macmillan.
Heap, James
1976 "What are sense-making practices?" Sociological Inquiry 46:107–115.
Hewitt, John P. and Peter M. Hall
1973 "Social problems, situations and quasi-theories." American Sociological Review 38:354–467.
Hymes, Dell
1979 "De-centering linguistics: A comment on Lemert." Theory and Society 7:306–318.
Israel, Joachim
1979 The Language of Dialectics and the Dialectics of Language. Atlantic Highlands, NJ: Humanities Press.
Jefferson, Gail
1973 "A case of precision timing in ordinary conversation." Semiotica 9:47–96.
Jones, Robert Alun
1977 "On understanding a sociological classic." American Journal of Sociology 83:279–319.
Langacker, R. W.
1976 "Semantic representations and the linguistic relativity hypothesis." Foundations of Language 14:307–358.
Lemert, Charles C.
1979a Sociology and the Twilight of Man. Carbondale: Southern Illinois University Press.
1979b "De-centered analysis: Ethnomethodology and structuralism." Theory and Society 7:289–306.
Lemert, Charles C. and Garth Gillian
1977 "The new alternative in critical sociology: Foucault's discursive analysis." Cultural Hermeneutics 4:309–320.
Lofland, John
1974 "Styles of reporting qualitative field research." The American Sociologist 9:101–111.
Maryl, William
1973 "Ethnomethodology: Sociology without society." Catalyst 7:15–28.
McCarthy, Thomas A.
1973 "A theory of communicative competence." Philosophy of the Social Sciences 3:135–156.
McHugh, Peter
1970 "On the failure of positivism." Pp. 324–335 in Jack Douglas (ed.), Understanding Everyday Life. Chicago: Aldine.
McHugh, Peter, Stanley Raffel, Daniel C. Foss, and Alan F. Blum
1974 On the Beginning of Social Inquiry. London: Routledge and Kegan Paul.
McSweeny, Bill
1973 "Meaning, context and situations." European Journal of Sociology 14:137–153.
Morris, Monica
1976 " 'Creative sociology': Conservative or revolutionary?" The American Sociologist 10:168–178.
Nisbet, Robert
1967 The Sociological Tradition. London: Heinemann.
Overington, Michael
1979 "Doing what comes rationally: Some developments in metatheory." The American Sociologist 14:2–12.
Piaget, Jean
1970 Structuralism. London: Routledge and Kegan Paul.
Pollner, Melvin
1974 "Mundane reasoning." Philosophy of Social Sciences 4:35–54.
Ricouer, Paul
1974 "Metaphor and the main problems of hermeneutics." New Literary History 6:95–110.

Ritzer, George
1975 Sociology: A Multiple Paradigm Science. Boston: Allyn and Bacon.

Scott, M. and Stanford Lyman
1968 "Accounts." American Sociological Review 33:46–62.

Silverman, David
1974 "Speaking seriously." Theory and Society 1:1–16 and 361–374.

Smith, Dorothy
1974 "The social construction of documentary reality." Sociological Inquiry 44:257–268.

Wieder, D. Lawrence
1974 "Telling the code." Pp. 144–172 in Roy Turner (ed.), Ethnomethodology. Middlesex: Penguin.

Wolff, Kurt H.
1959 "The sociology of knowledge and sociological theory." Pp. 567–602 in Llewellyn Gross (ed.), Symposium on Sociological Theory. New York: Harper and Row.

Zeitlin, Irving
1969 Ideology and the Development of Sociological Theory. Englewood Cliffs, NJ: Prentice-Hall.

RETIRED SOCIOLOGISTS' EXPECTATIONS FOR PROFESSIONAL IMMORTALITY: FURTHER DEMONSTRATIONS OF THE CONSTRUCTED NATURE OF REALITY*

· FRANK R. WESTIE   EDWARD L. KICK
Indiana University   University of Utah

An earlier study (Westie, 1973) attempted to demonstrate that the realities of sociologists, like the people they study, are socially constructed. The present study seeks to determine if these constructions undergo change with retirement. It was predicted that academicians would have "exaggerated" conceptions (as compared to other criteria of "reality") of their place in their field and the degree to which they will be remembered for their research and writings. The first study found that two-thirds of a sample of sociologists in Ph.D.-granting departments thought of themselves as possible candidates for inclusion among the "top ten individuals" in their specialty. A comparable proportion expected to be remembered for their research and writings, although many of these same respondents indicated that they had not heard of many of the people on a list of sociologists they were called upon to identify. The respondents were not told at the time that the list consisted of the names of all of the past presidents of the American Sociological Association. The retired persons reported on herein are remarkably similar to their fellow sociologists still on payrolls in their definitions of professional self and in the expectations for the survival of their research and writings beyond their own life spans.

A widely endorsed article of faith in sociology holds that reality, as any individual or whole groups of individuals experience it, is in many of its aspects socially constructed. Inductive, quantitative, empirical demonstrations of the socially constructed nature of reality remain rare, however, despite the recent increase in the number of sociologists who are willing to come forward to declare their allegiance to this primary principle of phenomenology.[1]

In an earlier study (Westie, 1973), sociologists were used as research subjects to illustrate the point that their realities, like those of the people they study, are socially constructed. It was predicted that academicians in general, and sociologists in particular, would have highly optimistic

[1] The publication of Berger and Luckmann's *Social Construction of Reality* (1969) was a major event in the sociological resurrection of phenomenology. We are particularly indebted to Berger and Luckmann's formulation, which provided us not merely with the phrase, "the social construction of reality," but which also influenced this research on "sociologists' construction of reality."

images of themselves regarding leadership in their field and the likelihood of their contributions surviving their own career spans. The findings supported these predictions: approximately two-thirds of the sample of sociology professors at Ph.D.-granting institutions regarded themselves as likely or possible candidates for inclusion among the top ten leaders in their specialty. A comparable number were optimistic regarding the likelihood that they would be remembered beyond their own career spans for their contributions. Both of these constructions stand in sharp contrast to other sets of realities as assessed (or constructed) by external tests. For example, more than 50% of the sample could not recall any of the works of more than one-third of the persons on a list of the past presidents of the American Sociological Association. Moreover, more than 90% of the younger sociologists studied had never heard of the names, to say nothing of the works, of 10 of the first 20 presidents of the ASA.

The present study seeks to determine what happens to these constructions of professional self when one is removed by retirement from the institutional setting and demands which, particularly in large departments, make such optimism not only possible but necessary.

Among the most important constructions of reality that people erect and nurture are those which have to do with the importance of one's occupational activities and the degree to which one's having performed these activities benefitted others. Most of us also hope that the consequences of our having lived and worked will survive beyond our own allotted time span, and that we will be remembered for what we have done.

Obviously, professors are not unique in their "strain to magnify" the importance and lasting nature of their occupational contributions. It would seem that physicians, lawyers, engineers, and others in high ranking occupations are also so inclined. But the present study proceeds from the assumption that this strain is particularly strong among university professors, largely because of the peculiar place of universities in the social system

and the peculiar place of professors in the universities.

Academicians are the beneficiaries of a range of perquisites that were, in the history of Western Society, reserved for aristocrats or those who enjoyed the patronage of aristocrats. Among these are the nonpower-related privileges which are more or less built into the academic life style: opportunities for self-expression, creativity, ego-expansion, meditation, a certain measure of prestige and honor, and the pursuit of ideas whether or not these ideas have any utilitarian consequences. It follows then, that if professors enjoy certain of these (historical) aristocratic perquisites, then, they *ought* to perform functions that are crucially essential and lasting. However, the normative notion that what professors do, particularly what they create, *ought* to be important and lasting, becomes translated in the development of the academic culture of legitimation into the cognitive conviction that what they do *is* important to the survival of the system and of lasting value.

It is not surprising, then, that much of the energies expended in universities are channeled toward the construction of illusions of greatness and the validation of these illusions—the greatness of individual faculty members, departments, and the university as a whole. That *some* individuals are great and *some* departments are great is not at issue here. Rather, the issue is whether *most* faculty members are great and *most* departments are great, by definition. And this is what we seek to further demonstrate.

If, as we had presumed, the magnification of professional self on the part of *non*-retired professors of sociology was to be understood as a product of institutional pressures and socialization to the academic culture of legitimation, then removal of the professor from the occupational setting through retirement should result in a decline in illusions. We did not expect dis-illusionment but, rather, movement in the direction of professional de-illusionment.

The disengagement theory of aging predicts opposite findings: with retirement

frequently comes detachment from the occupational setting which validates one's professional status. More important, perhaps, is the abrupt decline in interpersonal relationships with professional peers. If in retirement one only infrequently is responded to as professor or as sociologist, a very important, perhaps *the* most important facet of one's concept of self is threatened, even assaulted. Identity itself may be at stake. To the extent that any or all of this is true, we would expect the retired professor to have a greater need to emphasize and perhaps magnify past professional accomplishments.

Finally, there is the possibility that socialization to the culture of legitimation is so effective that one's optimistic images of professional self remain perpetually fixed.

METHOD

Letters were sent to the chairpersons of all Ph.D.-granting sociology departments in the United States requesting the names and addresses of all living sociology faculty members who had retired from their departments. The names of 158 retired sociologists were received from 111 departments. A questionnaire was mailed to each of these retirees and, with a second mailing, 92 usable questionnaires were returned.[2] Most of the respondents were over 70 years old and many over 80. Given this age distribution, it is reasonable to presume that many, if not most, of the

---

[2] These 92 cases account for 58% of the original mailing. Actually, 106 replies (or 67%) were received. The actual non-response rate cannot be determined, because the original list of 158 persons was considerably inflated. Chairpersons, who provided the names, lacked up-to-date knowledge about their retirees. The responses of friends and relatives informed us that five subjects were deceased or otherwise incapable of responding; three others were not retired or were not university sociologists; two could not be located due to inadequate addresses; two questionnaires were unusable due to failure of the subjects to follow instructions; two were refusals. We have no way of knowing how many of the 52 persons from whom we received no response were deceased or disabled. It seems likely that the number was substantial. The health of many of the retirees who were interviewed in depth was precarious.

non-responses were due to disability or death rather than due to factors related to the substantive issues of this research. In any case, we attempted to study the universe of sociologists retired from Ph.D.-granting institutions rather than a sample. In-depth interviews, some two to three hours in length, were conducted in the homes of 12 retirees in hopes of acquiring insights of the kind structured questionnaires cannot provide.

As in the first study, the respondents were asked to assess a number of aspects of their own careers, to indicate the degree to which they had made various types of professional contributions, and the likelihood of their being remembered for these. In addition, each retiree was asked to locate a specific peak in their career when they were most active professionally, and to indicate degree of agreement with the statement, "I was, at the peak of my career, one of the ten leading contributors to knowledge in at least one of my specialties." Respondents were also asked to indicate where they thought they ranked *now* in the minds of those still active in their primary specialty. Among other issues investigated were the following: the degree to which one felt their work was appreciated by colleagues and students; current professional activities; and early expectations for achievement versus actual achievement.

SELF-ASSESSMENTS OF PAST LEADERSHIP IN ONE'S SPECIALTY

The findings indicate that the images of professional self that sociologists construct and nurture in a lifetime in their profession are not ephemeral or transient, but have a persistence that would seem to match that of life itself. Over one-half (59%) of the 92 retirees studied regard themselves as having been among the ten leaders at the peak of their careers in at least one of their specialty areas (Table 1), while some 6% were undecided and thus did not discount such leadership as a possibility. Only one-third (35%) believe that such achievement was unlikely. In Table 1 the five response alternatives were collapsed into three categories:

TABLE 1. Expectations for Having Achieved Top
Ten in Specialty at Peak of Career

	Expectations			
	Low	Undecided	High	Total
Number	32	6	54	92
Percentage	35	6	59	100

"very likely" and "possibly" are classified as "high expectations," "not likely" and "very unlikely" as "low expectations." The "undecided" responses remain the same.

Those from larger departments have more optimistic assessments of professional self than those from smaller departments. While half of the retirees (Table 2) from universities granting fewer Ph.D.s believe they were in the top ten in their specialty at the peak of their careers, the comparable figure for universities granting numerous Ph.D.s is two-thirds (67%). Significantly, the proportion (two-thirds) of retired sociologists with high opinion of professional self is remarkably similar to the proportion of such optimists in the "active" sample previously studied.

Whether or not this finding indicates professional self-magnification on the part of retired sociologists depends on the number of sociology professors there were at the peak of the respondents' careers. If there were only, say, a few hundred, then there is probably little exaggeration involved in ranking one's self in the top in a standard specialty. It was precisely for this reason that we asked the retired sociologists where they thought they ranked *now* in the minds of professionally active sociologists.

TABLE 2. Expectations for Having Achieved
Top Ten in Specialty at Peak of Career, By
Number of Ph.D.s Granted in Last Ten
Years by Department

Expectations	Ph.D.s Granted		
	0–30	30+	N
High	50 (19)	67 (28)	47
Undecided	5 (2)	5 (2)	4
Low	45 (17)	28 (12)	29
Total	100	100	
N	38	42	80

TABLE 3. Expectations That One is *Currently*
Among Top Ten in Specialty

	Expectations			
	Low	Undecided	High	Total
Number	47	14	30	91
Percent	52	15	33	100

SELF-ASSESSMENTS OF CURRENT LEADERSHIP

Even more dramatic evidence of the persistence of high hopes is the fact that roughly half of the retired persons studied regard themselves as *still* among the top ten leaders in their specialty, or as "not without hope," that is, "undecided" (Table 3).[3]

Moreover, the evaluation of self as *still* among the top ten does not diminish with length of time since separation from one's occupational setting (Table 4). Actually, the number of persons who define themselves as currently among the top ten leaders in their specialty *increases* with the number of years since retirement. Sixty percent of those in the 7–30 years-since-retirement category are optimistic: 39% place themselves as *still* in the top ten, and 21% (undecided) apparently are "not without hope." The "top ten" and "undecideds" among those more recently retired (0–6 years) totals 40%. Thus, there is, with age, a 20% increase in the number of persons who apparently are not pessimistic.

EXPECTATIONS FOR PROFESSIONAL IMMORTALITY

The high expectations for professional immortality that were found among active sociologists do not fade with retirement.

[3] We frequently combined those who regard themselves as likely candidates for inclusion in the top ten with the "undecideds," because the choosing of the "undecided" response frequently tells us almost as much as the "top ten" choice. To take an extreme sample, if we were to ask a random sample of all biologists in America: "What is the likelihood of your receiving the Nobel Prize during your career?," and we found that, say, one-third answered "undecided" while another one-third say "quite likely," the significant finding is that two-thirds are "not without hope," or are unrealistically hopeful, given the number of scientists and the number of Nobel Prizes awarded.

TABLE 4. Expectations for *Currently* Being
Among the Top Ten Leaders in One's Specialty,
By Years Since Retirement

Expectations	Years Since Retirement		
	0–6	7–30	N
High	30 (17)	39 (13)	30
Undecided	10 (6)	21 (7)	13
Low	60 (34)	39 (13)	47
Total	100	99	
N	57	33	90

Actually, the retired sociologists are *more optimistic* about their professional survival than are their colleagues in mid-career. Almost two-thirds (62%) of the retirees are optimistic that their research and writing will survive their career span (Table 5). If we combine these with the "undecideds" (i.e., those not without hope), almost three-quarters of our sample are apparently not pessimistic about the survival of their research and writing. Again, the five categories of response were collapsed for presentation here: "very likely" and "probably" are defined as "high expectations," and the "doubtful" and "certainly won't" responses were classified as indicating "low expectations" for professional survival.

In an obvious sense, anything that one has published will survive as long as at least one copy of the published article or book exists. We found, however, in our pretests and in 12 personal interviews we conducted with retirees in various parts of the country, that they did not evaluate the survival of their research and writings in this simplistic sense, but rather as an identifiable and viable part of the sociological literature. This understanding of what the respondents meant by "survival" is also indicated by the retirees' answers to the question: "To what extent do you think you *ought to be remembered* [italics in original form] for your research and writing?" Fifty-five percent answered

TABLE 5. Expectations that Research and Writing
Will Survive Career Span

	Expectations			
	Low	Undecided	High	Total
Number	24	11	56	91
Percent	26	12	62	100

"somewhat" or "considerably" and 19% were "undecided." Thirty percent said "very little" and only six percent said "not at all."

It is clear that most of our subjects were talking about accepted specialties when they evaluated their achievements. When the retirees were asked about *current* leadership, the following two questions were juxtaposed: "What was (is) your primary specialty?" followed by, "Where would you say you rank *now* in your primary specialty in the minds of those still active in that specialty?" Were it not for the fact that there is much disagreement in the field as to just what the "major specialties" are, we would say that most of the specialties indicated were *major* specialties. In any case, by our classification, 90% of the specialties presented by the respondents as their major specialty were "accepted" specialties, while only five percent were "highly specialized areas" and five percent "idiosyncratic."

REALITY TESTS

No "reality test" is needed to assess the degree to which the subjects' assessment of likelihood of their having been (or still being) among the top ten leaders compares with external measures of such likelihood. By definition only ten people can occupy the top ten positions in each specialty and each of these is claimed by a great many people. Not only do a large number of one's fellow retirees claim these positions, they are already claimed by the majority of sociologists still on payrolls at larger universities, as indicated by our initial study.

Regarding the expectations for professional *survival* on the part of retirees, a reality test *is* required before one can say with any confidence that a group's expectations are overly optimistic or "exaggerated," and thus to be seen correctly as constructions of reality shaped by institutional pressures. Thus, all of our subjects were asked to indicate the degree to which they were familiar with the names and works of a list of distinguished sociologists of the past and present. The list, as previously indicated, consisted of the names of the 63 past presidents of the

American Sociological Association from 1906 to 1972, from Ward to Goode. As in the case of the first study, the retirees were not told that the people on the list were the past presidents.

The work of eight of the first 20 presidents (1907 to 1930) was unknown to over 50% of the retirees, and even the names of seven of these presidents were totally unknown to more than 40% of the retirees. This represents a substantial rate of forgetting, given the fact that the careers of a number of the persons in our sample overlapped those of a number of the first 20 presidents. In any case, a large number of our sample were contemporaries of many of the second ten presidents. The average age of the retirees studied was 73 years. Consequently, the average person studied was 26 years old in 1930, and thus many of them considerably older than 26 at the time. The remaining 43 presidents (1931–1972) fare much better, although the works of nine of these were forgotten, or never heard of, by at least 40% of the retirees. Whether this indicates a high rate of forgetting or of remembering depends on the eye of the beholder. The retired persons' memory of persons in the professional generation immediately preceding the beginning of their own careers is not remarkable. One is more impressed by the number of presidents they forgot. On the other hand, their memory of their contemporaries, the past 43 presidents, is quite good.

The most important reality test, however, is not how well the *retirees* remember the early and recent presidents, but, rather, the degree to which the *currently active sociologists* remember the past presidents. If the retirees are correct in their expectations of being remembered, then it is the currently employed sociologists who will have to do the remembering. If, as found in the previous study, a large proportion of the past presidents are forgotten, or were never heard of, by an appreciable proportion of the sample of active sociologists, then, "realistically" (from the standpoint of an external definition of reality) neither the retirees nor currently active sociologists should expect much in the way of professional immortality. (Reference here is to

the "average case," and not to the distinguished, well-known scholars in the universe of retirees studied. Some of them are past presidents themselves, and our findings indicate that they are well-remembered, as were other retirees who were, although distinguished, never presidents.)

The significant fact about this reality test is that so many of the distinguished leaders of the past were forgotten by so many of the living, and so many of the living forgotten by the living. Over half of the active sociologists had forgotten, or perhaps had never known, the works of half of the first 20 presidents, and among the younger sociologists, more than 90% had never heard of the name, to say nothing of the works, of 10 of the first 20 presidents.

Many of the persons on our list of retired sociologists were well-known nationally, and some of them still are. Yet they too, like the past presidents of the ASA in the initial study, are probably victims of the "new Columbus" syndrome, to use Sorokin's phrase (1956) in that they, the previous generation of sociologists, *must* be forgotten so that the current generation can rediscover America. But there is poetic justice at the end of this process: The forgetful sociologists of the present will, with rare exception, be forgotten by those who follow them, who in turn will be forgotten by those who rediscover *their* rediscoveries. The present research suggests that these "Sorokinian" speculations may be restated as plausible hypotheses.

SUMMARY AND PERSONAL OBSERVATIONS

This study began with the expectation that retired persons would have declining expectations for leadership and professional immortality with release from the setting that made participation in the academic culture of legitimation necessary. The findings suggest, however, that the retired professors are even more optimistic about professional self than the active population of sociologists. Not only do they, in the typical case, define themselves as having been among the top ten leaders, almost half of them believe they

still are. Finally, most of the retired sociologists expect to be remembered for their research and writing, when in fact many of the past presidents of the ASA were never heard of by a large proportion of the active sociologists studied.

It is perhaps an instance of reductionism to say that sociologists, whether active or retired, *personally* construct illusions of professional self in response to individual psychological needs. Rather, we think that it is a more tenable interpretation (now a hypothesis) to say that most academicians (including the authors) magnify their professional selves because they have been socialized to an academic culture of legitimation, which has itself been shaped and nurtured by the structural conditions within which departments and universities function.

One interpretation of all this is that socialization to the culture of legitimation is so complete that its effects remain with academicians throughout their lives. Just as professors remain very much professors in their life style, title, and self-identification, no matter how long they have been retired, so also, it would appear, do their evaluations of professional self remain.[4]

By way of personal observation, one of the most vivid impressions we were left with when we conducted in-depth interviews with retired professors is that neither age nor state of health appear to diminish the identity, "professor," nor the inquiring posture toward the world around one that is associated with that identity. In talking to retired professors, one becomes compelled more than ever to view the profession as a vocation in the strict sense of a "calling;" if one is truly called, one never truly retires. Perhaps one of the more important by-products of socialization to the academic culture of legitimation is high, lastingly high, morale. We can think of no better measure of occupational morale than the conviction that

one was, or is, among the very best in one's life's work.

It is worth noting, perhaps, that 74% of the retired sociologists indicated that if they had to choose a career again, they would still choose sociology, and of the 26% who would not choose sociology again, 24% would nonetheless choose an academic career. Only 2% would abandon the academy.

We are in no way cynical in our continued pursuit of this enterprise (we are now studying the self-evaluations of professors of English, and are preparing to study persons in other disciplines as well). Our purpose is to illustrate further the socially constructed nature of reality. It seemed appropriate to start this demonstration on sociologists, the very people who are inclined to dwell on the fact that reality is socially constructed.

To say that the professional self-images of retired sociologists (as well as active sociologists) are in some degree the product of socialization to the academic culture of legitimation is in no way a commentary on the importance of the contributions of the generation of retired sociologists we have studied. There is no way one can overstate the importance of the contributions of this generation of sociologists to the development of sociology as an established discipline. Nor do we regard as purely illusory the convictions of sociologists of the past or present, including ourselves, that what we do is important. All of us operate in this faith and surely this faith is not misplaced. But while we are confident that what we do in sociology is important, it is perhaps equally true that what we do is important because we do it.

---

[4] We choose not to interpret the finding that optimism appears to *increase* with length of retirement, because we are not sure that the differences in percentages are statistically significant. If we could be sure that the relationship is significant, then we would interpret it in terms of disengagement theory.

REFERENCES

Berger, Peter L. and Thomas Luckmann
  1966 The Social Construction of Reality. New York: Doubleday.
Sorokin, Pitirim
  1956 Fads and Foibles in Modern Sociology. Chicago, IL: Henry Regency Co.
Westie, Frank R.
  1973 "Academic expectations for professional immortality: A study of legitimation." The American Sociologist 8:19–32.

JEWISH SOCIOLOGIST: NATIVE-AS-STRANGER*

SAMUEL C. HEILMAN

Queens College and the Hebrew University of Jerusalem

The paper argues, by way of empirical example, that the long-accepted contention that participant-observation research can best be accomplished by strangers who become like natives is by no means the only or even best approach. Rather, natives can be trained to distance themselves from their own group, making use of inherently objective sociological and anthropological perspectives. Such natives-as-strangers are more likely to be cognizant of nuances in the cultural life being studied. Using the example of the author's research among Jews, the paper also outlines some of the risks of this native-as-stranger approach, foremost among them being the tendencies to take too much for granted and to become alienated from one's native group.

"The Jews," Franz Kafka once wrote, "have always produced their joys and their sorrows at almost the same time as the Rashi commentary relating to them . . ." (Robert, 1976:8). Indeed one might say the relationship was a symbiotic one. Without the Torah, the spiritual testament of the Jewish people, there could have been no Rashi commentary; and without the commentaries of Rashi, the Torah might in great measure have remained obscure and remote from the Jewish people.

Like their texts, the Jewish *people* have throughout their history been the subject of commentary and analysis, at first by the omniscient redactors of the Bible, then by the prophets and their scribes, and later by the exegetes and rabbis. While from the point of view of the sacred canon, social scientists and ethnographers who have followed, studied, and commented on the character of the Jewish people cannot be compared to these others, their roles might nevertheless be considered as being analogous. Although not motivated by religious impulse or design, these latter day observers and chroniclers are, like their spiritually oriented forerunners, fascinated by and convinced of the importance—"chosenness," if you will—of the Jewish people.

To observe the whole of a people requires, in the minds of many, an ability to distance oneself from any particular part of that whole, to avoid what has been called "the corrupting influence of group loyalties upon the human understanding" (Merton, 1972:30). As Francis Bacon, one of the most noted champions of this prerequisite for perspective, noted, our immediate surroundings, both social and psychological, dominate and hence seriously limit what we are prepared to perceive as well as how we perceive it. "Dominated by the customs of our group, we maintain received opinions, distort our perceptions to have them accord with these opinions, and thus are held in ignorance and led into error which we parochially mistake for the truth" (Merton, 1972:30–31). Only when one escapes from the peremptory embrace of the group can one begin to see it more clearly. In a sense, then, as both historical example and this sort of logic suggest, the chronicler of entire people must always be something of an outsider or stranger to them. He must acquire some distance in order to gain some objective perspective.

Distance and the role of the stranger do not however necessarily suggest that one is *only* or even *primarily* an outsider. Rather, the stranger, as Georg Simmel long ago noted, is a paradoxical creature. He or she "is an element of the group itself," whose membership nevertheless

"involves both being outside and confronting it" (Levine, 1971:144). Although an insider to some extent, the stranger, either through personal perception or from the point of view of other insiders, is somehow also an alien. As such, the stranger is able to take something of an objective, unbiased attitude toward group life and activity, an attitude that makes him or her not only a logical observer, but also, often, the recipient of far reaching revelations and confidences from insiders who seek an 'impartial judge.' The stranger is insider enough to comprehend these messages and reports yet outsider enough to judge them objectively.

The objective attitude inherent in the stranger's role thus "does not signify mere detachment and remoteness, but is a distinct structure composed of remoteness and nearness, indifference and involvement" (Levine, 1971:145). But, because the stranger's position and perspective is in fact an amalgam of oppositional views, it is necessarily a fragile and at times volatile way of life. The ineluctable contention of these opposing perspectives for even temporary dominance often leave the stranger divided between a desire to become assimilated as a complete insider or else freed from any bonds with the group in question. He or she is in a sense not altogether unlike the "agent provocateur," whom sociologist Gary Marx describes as one who on the one hand is a "trusted member of the group," but who at the same time is always sufficiently estranged to be prepared for "betrayal and deception," and who therefore feels torn between feelings of sympathy for and alienation from the group in question. The stranger too cannot be content with the duality (or, if you will, duplicity) inherent in this position.

One can see, I believe, without much difficulty, parallels between the position of the stranger and that of the previously referred to commentators and chroniclers of Jewish life. They too have had to learn both to stand apart from and to keep their attention riveted to that life. In particular, the social scientists and ethnographers who, via participant observation—learning by watching while doing—seek to penetrate the life and consciousness of a group they are studying, while at the same time maintaining a rigorous and disciplined distance from it, seem to fit neatly into the role of stranger. Exhorting one another to become participants, to "get inside our subject's heads," and make themselves aware of "the complete range of unconscious possibilities" in those lives, they also strive to maintain "objectivity about [that] way of life" and "escape from its habits of thought" (Frake, 1964). Only thus, they believe, can one personally learn where to draw the line between subjective and objective reality, thereby learning the truth about another's way of life.

While literature, both biblical and secular, has long since recognized the capacity of natives to become strangers-commentators-chroniclers among their own people, social science and the academic world have been far more reluctant on this score.[1] Traditionally, the acceptable approach to participant observation has been for complete outsiders to learn to be like natives, all the while retaining a transcendent strangeness or distance and the perspective it provides. The implication was that participation could be learned, but detachment, distance, and the escape from habituated activity was something that could not be learned. An outsider could be made into an insider, but never the reverse.

In a now well-known paper on this subject, Robert Merton called this the "Outsider Doctrine." It holds basically that insiders have a structurally imposed incapacity to comprehend their own group because, having been socialized into its way of life, they no longer are capable of being sensitive to the grammar of its conduct and nuances of its cultural idiom in any objective way (Merton, 1972:30–36). The insider's understanding is empathic,

---

[1] There have of course been a number of cases of natives studying their own culture, for example: Dozier (1970), Kenyatta (1953), La Flesche and Fletcher (1970), Ortiz (1969), Seeley et al. (1956), Strehlow (1953), to name but a few. Among Jews although Gutwirth (1970), Kranzler (1961), Poll (1969), Rubin (1972), and Shaffir (1974) were Jews studying Hassidim, they were not Hassidim themselves. In any event the native participant observer is the exception rather than the rule in social science literature.

but therefore also cannot be translated into terms outsiders could comprehend. Insiders can be advocates but never commentators and analysts according to this doctrine. To paraphrase Max Weber: "One cannot be Caesar in order to understand Caesar."

This doctrine was well suited to those anthropologists and ethnographers who left their native European or American milieu in order to live amongst and learn about 'exotic' peoples. Their approach, basing itself on their implicit cultural and moral superiority over those whom they studied, presumed that an outsider could learn to be an insider, and that no insider alone could accomplish the task of explanation as well. The detailed reports that came out of such efforts seemed to indicate that indeed, strangers could become like natives. Moreover, while these researchers seemed to have lived among those about whom they wrote, they had not gone so far as to "go native" and lose all contact with their own groups of origin.

When one reads the diaries and field notes of many of these pioneer anthropologists, however, one becomes aware of the many obstacles that stood in the way of their learning the way of natives and of their repeated failures (see, for example, Wax, 1972). One reads of natives laughing at one's inquiries and conclusions, of "bewildering confusion," limitations, unfavorable conditions, and gaps in understanding (Boas, 1966:4). This is not to deny the genuine greatness of these early anthropological accomplishments, but rather to stress the immensity of their difficulty. Learning a new language and its nuances of meaning, the importance of kinship and rules of association, patterns of belief and their effect on behavior, the native's sense of time and space—to mention but a few of the more striking obstacles—were matters not easily overcome by even the most prepared neophyte.

As long as few natives ever read or publicly commented upon the resultant chronicles and analyses, the researchers could assert the authenticity of their reports and commentary with little risk of denial. When, however, these techniques of participant observation became utilized by sociologists who studied groups whose natives could and did comment upon the findings, denials from the latter became far more frequent (consider, for example, Whyte, 1955; or Vidich and Bensman, 1968:317–476). Indeed, by the late nineteen sixties and into the seventies, when widespread modernization had significantly decreased the number of exotic primitive cultures available for study and those remaining—primarily in "Third World" countries—were not receptive to alien social scientists who were generally identified with the degradations of colonialism, an "Insider Doctrine," which asserted that "you have to be one to understand one" became ascendant in many circles" (Merton, 1972:15; Shokeid and Deshen, 1974:50).

Without judging the veracity of this extreme position, or lapsing into what has been called the "egocentric predicament," which supports the belief that "all one *really* knows is one's subjective experience," one must however note the awesome difficulties in learning how to be like a native (Merton, 1972:14n). Strangers who strive to become like natives, and who necessarily work hard at the effort, quite often discover that the harder they work to efface their singularity, the less are natives inclined to regard them as natives. This is because "everyone knows that a native requires no special effort to conform to his country's customs and habits of thought, whereas the foreigner gives himself away by his need to explain and understand not only complicated matters, but the simplest trifles, the thousand nothings that are said and done mechanically in every moment of daily life" (Robert, 1976:17). In their most candid and open moments, almost all the early anthropologists and many contemporary sociologists and ethnographers had had to admit to this flaw in their efforts at participation.

If there are significant difficulties for strangers who wish to become like natives, is it then possible for natives to become strangers? Can they learn to distance themselves intellectually, emotionally, and socially so as to look upon the taken-for-granted world in which they live as insiders, as if it were new to them? Can

natives learn to question all that they have already unconsciously learned, so that they can reconstruct and explain it for the benefit of outsiders? Finally, can natives learn to take the trouble to inquire about the obvious without at the same time becoming so distant from other natives that they become viewed as outsiders?[2]

Although Bronislaw Malinowski, a pioneer of participant observation, was speaking about the possibilities of outsiders becoming insiders, his words might easily be fitted to the question of an insider learning to take the stranger's point of view. Writing in 1922 he commented: "I am not certain if this is equally easy for everyone . . . but though the degree of success varies, the attempt is possible for everyone" (Malinowski, 1922).

These questions have had particular meaning for me, both as a Jew and as a sociological commentator on Jewish life.

My own work has in great measure been focussed around the examination of the behavior and character of American modern Orthodoxy, a brand of Judaism which seeks at once to remain steadfast in its commitment to adhere faithfully to the beliefs, principles, and traditions of Jewish law and observance, without being either remote from or untouched by life in the contemporary secular world. This work begins from the premise that a native *can* become a stranger for whom the entire gamut of native behavior needs to be explained.

Raised and living most of my life among modern Orthodox Jews, I have been counted and treated as a cognitive, affective, and social insider as long as I can remember. There seemed nothing extraordinary to me about these Jews, nothing especially significant in sociological terms about them. Like most natives, I

---

[2] Until now I have described *insiders* and *outsiders, natives* and *strangers*, as if they were absolute categories. They are not, but I have stressed and will continue to emphasize their polar qualities in order to underscore my particular research predicament. More precisely, however, there are intermediate possibilities. Although a paper on methodological dilemmas is not really the place to elaborate these categories (something I have tried in part to do elsewhere (Heilman 1976; 1977)), a brief sketch of some possibilities is in order if only to suggest the complexity of the research situation.

Insider and outsider may be considered both in terms of type and of degree. With regard to the former there are at least four possibilities: one may be an insider or outsider from the (1) cognitive, (2) affective, (3) social, or (4) temporal points of view. With regard to degree, one may occupy any of these statuses either as newcomer, neophyte, or complete member. Some examples might be helpful.

Consider, for example, one who is a cognitive outsider but an affective insider: the Orthodox Jewish biologist who intellectually does not comprehend the universe as do his fundamentalist brethren, but still feels close to them emotionally. Conversely, this same person may share a cognitive unity with other biologists, regardless of their religious persuasion, but still may be affectively alien from them. Or, to take another example, one may be socially aligned with a particular group, but remain cognitively and emotionally estranged from the very same group. The Gentile husband at a gathering of his Jewish in-laws is a social insider, but may remain a cognitive and affective outsider. Or to offer one final example, one may be a complete insider— cognitively, affectively, and socially—but only temporarily. The guest who, by virtue of his host, enters completely into the latter's world, but nevertheless later leaves it completely, is a case in point.

These illustrations of types of insiders and outsiders by no means exhaust the possibilities or even the permutations of the four types listed. The distinctions of type should however by now be sufficiently clear to allow the reader to complete the picture.

In addition to differences of type, there are also differences of degree. Newcomers are no longer foreigners but rather status occupants (either as insiders or outsiders) on a trial basis. They remain capable of leaving or being rejected by other status occupants. Converts from Judaism, particularly in nineteenth century Germany, often found themselves newcomers to Christianity who, in the final analysis, never succeeded in moving beyond this degree.

Next one may become a neophyte, one who may share in the cognitive, affective, or social life of the status, but because of relatively recent involvement continues to retain ties to another group and associated status. These ties are enough to at least partially distance one from complete insiders whose commitment to the group and associated status is, at least in principle, total. The second generation immigrant is often an example of the neophyte, while the third has been described as becoming a complete insider.

Finally, there are fellow travelers. These are marginal people who are neither insiders nor outsiders, not seeking to enter or leave either of these statuses, but who are nevertheless associated with the group in question. These are people who would probably say something like: "I am not one, but some of my best friends are."

To be sure, the various types and degrees of insider and outsider described above must also be understood as ideal types. Empirical reality is always far more complex. Still, if one considers the methodological straits that one enters by virtue of his being a type of insider and a type of outsider with different degrees of belongingness in each, one begins to sense the staggering difficulties which stand in one's way.

took for granted the order and meaning, the social construction of reality, that constitutes modern Orthodox life.[3] The countless details of daily existence in the Orthodox Jewish community had become part of my natural behavior as well, and lacked a problematic character that is often the key stimulus for ethnographic observation. Orthodox Judaism, and in particular its synagogue life—the subject of my subsequent inquiry (and in fact the title of the book coming out of that inquiry)—was for me a religious and communal way of life rather than a subject for social research.

By chance, though perhaps as well out of underlying passion, my sociological imagination and religious inclinations— two spheres that I had always kept segregated—became united. Offered a grant to prepare an ethnography of a modern American Orthodox synagogue, in part because I had ready access to such a setting as well as the necessary background, I seized the opportunity. I elected to use the participant observer approach in my work because I had had good experience with it in other research, and because I was convinced of its efficacy in yielding accurate information. But having chosen this approach, I came face-to-face with the questions of native versus stranger.

There were epistemological questions: would my own comprehension and subsequent explanations be accurate reflections of what I had seen, or would they be projections of my own view, ingrained over time, and twisted to suit my immediate purposes at hand? There were ontological questions: was it possible for anyone—even a native—to comprehend the "thoughts, feelings, acts, assumptions and cognitions" of another (Davis, 1973:333)? There were methodological doubts: could I ever observe the action unsullied and unaffected by my presence and participation in it? These, however, were the same questions confronting all participant observers, whether or not they began as natives.

---

[3] For a fuller consideration of the character of modern Orthodox Jews, see Heilman (1976; 1977; 1978).

For me, however, there were special difficulties beyond these standard ones. Most prominent among these was the emotional one. The synagogue I would study was my own. I had been a member there for several years and was known and trusted by its members; many were my friends. Could I transform friends into informants and my life activity into research data? Would I be able to find the right questions to ask when I already thought I knew all the answers? And even if I succeeded in dissipating my lack of astonishment with this community's life and learned to ask questions, how would others who believed I already knew the answers respond? Would they believe in the genuineness of my inquiries? Would I not in the process run the risk of alienating myself from my own community? Would I come to be a cognitive, affective, and social stranger to the Orthodox Jewish community even as I became by virtue of my research activity an insider to the academic one?

There was also the question of whom I owed the greater allegiance: the academic or Jewish community. Was my first responsibility to the academy as a social scientist whose every scientifically grounded question demands an answer, whose research needs rather than personal values or predilections must be the guides for his action, and who must constantly guard himself against the dangers of over-rapport with those whom he studies? Or was I first an Orthodox Jew, a member of the synagogue community, beholden to his congregation and his faith, and willing to limit himself as do all insiders by the dictates of tradition and Jewish law? Was, for example, the Sabbath to be a rich source of data or a day of rest? To these questions and others that struck me precisely because I began as a native, there were no easy answers.

Nevertheless I began my study, and have carried on since, studying people that were my own. I noted and continue to take notice of everything I can, taking the cognitive perspective of a stranger, always writing my analysis as if for the comprehension of outsiders. There are details but no trifles. The questions posed to me by outsiders, often my academic

colleagues, serve as cues for my observations. Yet while I seek to take the role of stranger to explain the Orthodox Jewish world to outsiders, to convince them of the sociological richness of this group of people, its chosenness, I retain the advantages of an insider. I know where to look for the action and what questions to ask.

Indeed certain observations and the insights they provide seem possible precisely and only because I am informed as an insider. Perhaps an example can best explain this advantage. During my research in the Orthodox synagogue, I gradually came to realize that issues of social status were ineluctably linked with expertise in ritual matters and social competence in the synagogue setting. Moreover, it seemed very important to the synagogue members to find ways to display—albeit often in a subtle and understated way—their expertise, competence, and status. One way of accomplishing this was, at the proper time of the month, by the worshippers singing aloud in the midst of their silent prayers the opening words of "*Yale veyovo*," the prayer celebrating the new month and new moon. By so doing one could, while manifestly cuing or reminding others within earshot to add this special prayer, also latently and subtly use the occasion to display one's liturgical competence: only those in the know remember to recite "*Yale veyovo*."[4]

Had I had to make use of informants in this case, they would have had to provide reports much more complicated and expressive than a mere translation of the words "*Yale veyovo*." They would have had to explain the symbolic as well as the denotative meaning. While a denotative meaning would have revealed that the liturgy was adjusted in celebration of the new moon, only a symbolic explanation

would have shown how the liturgy was used by insiders as a status enhancement device. To expect informants to provide this information is tantamount to asking them to become social researchers themselves. Moreover to expect an outsider to take note of this event without a familiarity with Hebrew, the liturgy, and patterns of behavior in Orthodox synagogues would be equally unrealistic. By combining familiarity with detachment, I was able to "discover" something I had always "known." Only the insider/outsider, stranger has access to this sort of information.

Strangeness and distance, however, as already suggested, are states of being not easy to control. The effort to examine native life as if I were a stranger has made the community respond to me in some measure as if I genuinely were one. In some cases friendships have become strained, and allegiances that insiders were once ready to assume on my part are no longer always presumed. At first jokingly, but later with a trace of anxiety and even resentment, people put up their guard for fear that "Heilman will write a book about us." The presence of the neophyte stranger has made many of the natives around me try to take my perspective as well, sometimes telling me what I ought to focus my analytic eye upon.

Since the publication of my book and subsequent articles, I have begun to receive suggestions of communities and practices to study. At the same time, however, I also have begun to receive inquiries from Orthodox Jews about the genuineness of my Orthodox credentials. One rabbinic reviewer of my book, for example, took special pains to point out a minor mistake in my explanation of a Jewish law, and used this to question my expertise as an Orthodox Jew. Students who read my work later tell me that they assumed I had become Orthodox only during the period of my research, but could not afterward any longer perceive myself an insider to the Orthodox community. To be sure, I cannot be sure any longer that this is not the case.

Perhaps the most dramatic evidence of my having become considered something

---

[4] Milton Himmelfarb has suggested to me somewhat persuasively that the elaborate recitation of *Yale veyovo* does not so much positively enhance the social status of the speaker as it helps avoid the negative status of ignoramus. The point of one's needing to be an insider/outsider to catch this observation is however not denied by this interpretation, since even according to Himmelfarb this recitation has a symbolic character which only a combination of perspectives reveals.

of a stranger came most recently. Early in 1978, I contributed an article about the contemporary condition of modern Orthodoxy to a magazine that was planning a special edition on the "Varieties of Religious Experience." Shortly after the publication of my piece, I received an invitation from the program chairman of a suburban, modern Orthodox synagogue to speak to its congregation. He had read my article and thought I might have something to say to his congregation. I accepted and the arrangement seemed settled. A few days later I received another call from the program chairman. He had shown my article to his rabbi, and now the rabbi was afraid of my coming to speak to the congregation. He was afraid that I was a recreant, one who once might have been an Orthodox Jew, but, judging from the article in question, was not one any longer. The program chairman hesitated to express the ultimate question: could I be trusted, the rabbi wondered, to speak before a community of Orthodox Jews without turning them away, as he believed I had, from the ways of the Torah? Was I *at the very least* still emotionally tied to Orthodox Jewry?

The question struck me deeply, not so much because I cared whether or not a particular rabbi was convinced of my Jewish credentials or identified me with 'false prophets,' but because it signified how far I had come since my decision to focus my sociological eye on my own way of life and the community from which I had originated. The turn inward, it seems, has catapulted me to the other side, so that I can no longer assume my own native status. If the risk for strangers who try to become like natives is that they will irretrievably "go native," the danger for natives who try to look at things as would a stranger seems to be that they will "go stranger," no longer able to count on their acceptance as natives by other natives.

Ironically, by merging my academic interests with my communal life, I seem to have assured my estrangement from that community. Perhaps the traditional suspicions which arise when one is studied or under scrutiny guarantee hostility between researchers and their subjects. In any event, one might suggest that the gaps

that divide the academy from the community are not always solely the making of the academy.

Not only does one begin to wonder whether other natives will cease to accept one's loyalty and belongingness, but one begins to doubt oneself. Undeniably my perspective on the Orthodox Jewish community has changed at the same time that I have become increasingly accepted into the academic community of social scientists. If it is the case, as some have suggested, that "a Jew is a Jew when he is with other Jews," because for Jews "isolation is intolerable and life is with people," involvement in communities other than the Jewish one necessarily distance one from the latter (Rotenstreich, 1973:56; Zborowski and Herzog, 1952:189). Accordingly, often I feel like a marginal member of the Jewish community.

The characteristic feelings of the marginal man—"insecurity, ambivalence, excessive self-consciousness"—not altogether unrelated to the techniques of participant observation, in some measure seem to be by-products of my sociological investigations (Goldberg, 1941:53). This is a sort of "reverse-*Judenschmerz*," where the pain comes not from one's inability to participate in the outside world because of one's Jewishness, but rather from one's alienation from the Jewish world because of participation in a world and activity that is not strictly speaking parochial. Without overemphasizing the emotional strain, one might simply note that while marginality facilitates analysis and observation, it also engenders no small degree of dislocation and discomfort for Jewish academics like me.

In spite of the suspicions generated by natives becoming strangers, there is also the possibility of their becoming considered "experts." As such, they often find themselves being requested to explain insiders to outsiders and to themselves. When the audience is composed of outsiders, and explanations contain revelations about the inevitable imperfections of the native group, the sociological interpreters, the erstwhile natives, often feel as if they, have betrayed a confidence. This feeling is often reinforced by reactions from the na-

tive audience. Several times I have been asked by Orthodox Jews who have read my work why I felt the need to reveal so much about them that was less than favorable to outsiders, who could not be expected to respond with any sympathetic understanding.

On the other hand, those outsiders to Orthodox Judaism who are insiders in the academic community often have offered a sympathetic ear and a respite from the growing feelings of alienation which characterize the researcher's life in the Orthodox community. One should not be surprised, therefore, to learn that often natives-cum-strangers find it easier to report their research findings to other researchers than to members of the community studied. So it was for this researcher.

Explaining natives to themselves is quite another matter. Like portrait artists who find their subjects' image of themselves different from the one that is on the canvas, the person explaining a people to itself runs the risk of disturbing the sense of themselves with which they have already come to terms. When commentators are ones who purport as well to be insiders, they may be judged either as not being genuine insiders (for how else could they be so mistaken), or else suspected of manipulating the facts for professional or personal reasons. Even the most sympathetic audiences signal their doubts about one's interpretations—particularly when these touch on matters close to group identity and integrity—by a kind of symbolic "wink" which suggests a feeling of: we-know-you're-wrong-but-because - we're - friends - we - won't - say-anything - about - it.

There are also audiences made up of non-Orthodox Jews who as Jews feel a sense of rapport with, and hence an interest in, their Orthodox brethren. At the same time their non-Orthodoxy allows for a sense of cognitive or social distance and discrimination. Out of the rapport, born of emotional, ethnic, or avuncular ties, comes an enthusiasm for the reports on Orthodoxy. Out of the distance, however, there may come a hostility which helps justify the audience's non-Orthodoxy. Latching on to some item in the report that confirms a negative image of Or-

thodoxy, these sorts of audiences often manage to turn the reporter into an involuntary ally. He or she, after all, proved they were right about Orthodox Jews all along. Needless to say, these audiences often leave the native-as-stranger in doubt about the ethics of his or her research and the value of his or her estrangement. They too, like the audience made up of natives, latently encourage the native-as-stranger researcher to return to the neutrality of the academy.

Everett Hughes once noted: "The most important things about a people are the things they take for granted" (Whyte and Braun, 1968:120). These, perhaps most available to the insider, are, as already suggested, by no means easy to discover. Yet, as Hughes has also suggested: "It is doubtful whether one can become a good social reporter unless he is able to look, in a reporting mood, at the social world in which he was reared" (Hughes, 1960:xi-xii). As one who has tried to do this, I have at once seen things available to no one else's eyes, but at the same time paid a price for my perspective. At times the price seems too high, while at other times—particularly when I can stimulate others to think about the implications of what I have seen—well worth it. While generalizing from my experience may be intellectually risky, it may nevertheless serve to caution those Jewish natives and academics who believe that self-conscious commentary, observation, and analysis is the easy path to explaining the Jewish community.

REFERENCES

Boas, Franz
  1966 Kwakiutl Ethnography. Chicago: University of Chicago Press.
Davis, Fred
  1973 "The Martian and the convert: Ontological polarities in social research." Urban Life and Culture 2:330–342.
Dozier, Edward
  1970 The Pueblo Indians of North America. New York: Holt, Rinehart & Winston.
Frake, C. O.
  1964 "Notes on queries in ethnography." American Anthropologist 66(#3, Part 2):132–145.
Goldberg, Milton
  1941 "A qualification of the marginal man theory." American Sociological Review 6:52–58.

Gutwirth, J.
1970   Vie Juive Traditionell. Paris: Les Editions de Minuit.

Heilman, Samuel C.
1976   Synagogue Life: A Study in Symbolic Interaction. Chicago: University of Chicago Press.
1977   "Inner and outer identities: Sociological ambivalence among Orthodox Jews." Jewish Social Studies 39:227–240.
1978   "Constructing Orthodoxy." Society 15(4):32–40.

Hughes, E. C.
1960   "Introduction: The place of field work in social science." Pp. xi-xii in Buford Junker, Field Work. Chicago: University of Chicago Press.

Kenyatta, Jomo
1953   Facing Mount Kenya. London: Secker & Warburg.

Kranzler, G.
1961   Williamsburg. New York: Feldheim.

La Flesche, Francis and A. Fletcher
1970   The Omaha Tribe. New York: Johnson Reprint Co. (1922).

Levine, Donald (ed.)
1971   Georg Simmel: On Individuality and Social Forms. Chicago: University of Chicago Press.

Malinowski, Bronislaw
1961   Argonauts of the Western Pacific. New
(1922)   York: E. P. Dutton.

Merton, Robert K.
1972   "Insiders and outsiders." American Journal of Sociology 78:9–47.

Ortiz, Alfonso
1969   The Tewa World. Chicago: University of Chicago Press.

Poll, S.
1969   The Hasidic Community of Williamsburg. New York: Schoken.

Robert, Marthe
1976   From Oedipus to Moses. R. Manheim (trans.). New York: Doubleday.

Rotenstreich, Nathan
1973   "Emancipation and its aftermath." Pp. 50–58 in D. Sidorsky (ed.), The Future of the Jewish Community in America. New York: Basic Books.

Rubin, Israel
1972   Satmar: Island in the City. New York: Quadrangle.

Seeley, J., R. Sim, and E. W. Loosely
1956   Crestwood Heights. New York: Basic Books.

Shaffir, W.
1974   Life in a Religious Community. Toronto: Holt, Rinehart & Winston.

Shokeid, Moshe and Shlomo Deshen
1974   The Predicament of Homecoming. Ithaca, NY: Cornell University Press.

Strehlow, T. G. H.
1947   Aranda Traditions. Melbourne: Melbourne University Press.

Vidich, A. J. and J. Bensman
1968   Small Town in Mass Society (rev.). Princeton, NJ: Princeton University Press.

Wax, Murray
1972   "Tenting with Malinowski." American Sociological Review 37:1–13.

Whyte, W. F. and R. R. Braun
1968   "On language and culture." Pp. 119–138 in H. S. Becker, B. Geer, D. Reisman, and R. Weiss (eds.), Institutions and the Person. Chicago: Aldine.

Whyte, W. F.
1955   Street Corner Society (2nd ed.). Chicago: University of Chicago Press.

Zborowski, Mark and E. Herzog
1952   Life is With People. New York: International Universities Press.

JOURNAL PRESTIGE AND QUALITY OF SOCIOLOGICAL ARTICLES*

JAMES J. TEEVAN

The University of Western Ontario

Following the Glenn (1971) research design for evaluating the prestige of sociological journals, actual journal articles were evaluated. The data reveal both a slightly different ordering of journal prestige and greater within than between journal variation.

Everyone knows that a scholarly article should be judged only by its own merits. But in reality, many evaluators, especially deans and department heads, but also colleagues, may not adhere to that ideal, and instead judge a publication's worth by the reputation of the journal in which it appears. Inadequate knowledge of subject areas, time pressures, laziness, and even jealousy may interact to encourage such shortcut alternative judgments. Because so many people judge individual articles by their journal affiliations, Glenn (1971) even conducted a study of the prestige of sociology journals, reasoning that such evaluations should be based on a discipline-wide assessment of journal prestige.

The purpose of the present paper is to test and reemphasize the notion that there is great unevenness in the quality of articles within any journal. In order to demonstrate this variability, the Glenn methodology (listed in parentheses below) was repeated with one major and several minor modifications. Whereas Glenn requested that respondents evaluate journals, for this study the respondents were asked to read and to evaluate journal articles. Articles were mailed in 1975 to a random sample of 1050 (250) professors and associate professors in departments of sociology with M.A. or Ph.D. programmes (Ph.D. only) listed in the *Guide to Departments of Sociology, 1974* (1969). Articles in the *American Sociological Review* (the *ASR* itself) were used as the standard reference. A weight of 10 was

arbitrarily assigned to an article in the *ASR*, (*ASR* itself) so that an article (journal) only half as important as an *ASR* article (journal) should be assigned a weight of five, an article (journal) twice as important a weight of 20 and so forth.

Six "general" sociology journals were chosen from across the Glenn prestige scale (Glenn ratings in parentheses): *American Journal of Sociology* (9.6); *British Journal of Sociology* (7.8); *Canadian Review of Sociology and Anthropology* (6.4); *Sociological Quarterly* (6.1); *Sociology and Social Research* (5.9); and *Pacific Sociological Review* (5.7). Their ordinal ranks out of 63 on the Glenn scale are 1, 4, 29, 37, 41, 45. Within each journal, five articles were randomly selected (purely statistical and technical methodological selections were omitted). Each article was offset, after identifying marks had been deleted, and sent to 35 reviewers.

RESULTS

Five measures of prestige were computed for each article and for each journal: (1) Percent response rate. Some articles were evaluated more frequently than others. Perhaps certain titles or subject areas make some articles more attractive (prestigious?) than others. (2) Percent of articles *recognized* by reviewers and thus returned without evaluation. If the source of an article is remembered, perhaps it is a more important or prestigious article. (3) Mean prestige rating assigned by evaluators. These scores are the ones most similar to Glenn's prestige ratings. (4) Median prestige rating. (5) Percent of ratings above ten, the arbitrary *ASR* standard.

Overall journal ratings, calculated by a

weighted mean of their articles' scores are presented in Table 1, while the data for each article appear in Table 2. For both tables the journals are listed in order of their original Glenn prestige rankings. Inspection of Table 1 reveals that the total response rates vary across journals from 34.5% to 45.6%, an 11.1% difference, and that overall, the best rated journal, as measured by its response rate, is SSR at 49.1%, followed closely by PSR and AJS. CRSA, SQ and BJS are ranked fourth, fifth and sixth. On percent of articles recognized, column D, AJS articles are most frequently recognized, but the differences among journals are small.

Looking at median ratings, column I, SQ and AJS are ranked highest, at 8, while the other journals are clustered at 6. For percentage of ratings above ten, SQ is again the highest rated journal (21.7), BJS second, and AJS third. PSR, CRSA and SSR are closely ranked and follow below the AJS.

The mean rating of a journal's prestige, derived directly from the actual ratings of its articles (column G), is more comparable to Glenn's measure. Examination of these data confirms the earlier findings that evaluating articles instead of journal titles results in a rank ordering of journal prestige different from Glenn's. Overall the highest rated articles and thus journals are SQ, and then AJS. The other four, PSR, CRSA, SSR, and BJS are all lower rated and approximately equal. SQ (and PSR) thus fare better while BJS fares worse in comparison with Glenn's scale.

The purpose of the present paper, however, is not to replace the Glenn scale with yet another scale, but to demonstrate the variability in prestige ratings of different articles within the same journal. Examination of Table 2 supports this contention. For example, within journals the differences between the high and low response rates (column F) are: AJS 29.3%, BJS 20%, CRSA 30.6%, SQ 15.5%, SSR 18.8%, and PSR 16%, compared to the journal range of 11.1%. On the dimension, percent of the articles recognized, the differences between high and low recognition rates within journals ranges from 11.8% for AJS and SSR to 5.7% for PSR. The overall journal range on this variable

TABLE 1. Journal Prestige Based on Evaluations of Individual Articles

Journal and Glenn Rank	No. sent out A	No. could not review B	Effective N (A-B) used in D,E,F C	% recognized articles D	% evaluated articles E	Total % response rate (D+E) F	Mean prestige rating G	Standard deviation of G H	Median prestige rating I	% ratings above 10 (as % of E) J
AJS (9.6)	175	4	171	4.7 (8)	39.8 (68)	44.4	8.7	.7	8	11.8 (8)
BJS (7.8)	175	1	174	2.3 (4)	34.5 (60)	36.8	6.4	2.2	6	13.3 (8)
CRSA (6.4)	175	4	171	1.8 (3)	35.1 (60)	36.8	6.8	1.2	6	5.0 (3)
SQ (6.1)	175	2	173	2.3 (4)	34.7 (60)	37.0	9.2	1.7	8	21.7 (13)
SSR (5.9)	175	4	171	3.5 (6)	45.6 (78)	49.1	6.5	1.4	6	5.1 (4)
PSR (5.7)	175	5	170	4.1 (7)	42.4 (72)	46.5	6.8	1.3	6	6.9 (5)

TABLE 2. Evaluation of Prestige of Journal Articles

Journal	Article No.	No. sent out A	No. could not review (anthropologists, social workers, on sabbatical) B	Effective N (A−B) used in D,E,F C	% recognized article D	% evaluated article E	Total % response rate (D+E) F	Mean prestige rating G	Median prestige rating H	% ratings above 10 (as % of E) I
American Journal of Sociology	1	35	2	33	3.0 (1)	60.6 (20)	63.6	8.2	8	10.0 (2)
	2	35	1	34	2.9 (1)	41.2 (14)	44.1	8.3	8	0
	3	35	0	35	5.7 (2)	34.3 (12)	40.0	8.2	8.5	8.3 (1)
	4	35	0	35	0 (0)	34.3 (12)	34.3	9.7	9	33.3 (4)
	5	35	1	34	11.8 (4)	26.5 (9)	38.2	9.6	10	11.1 (1)
British Journal of Sociology	1	35	1	34	2.9 (1)	41.1 (14)	44.1	9.7	10	35.7 (5)
	2	35	0	35	0 (0)	25.7 (9)	25.7	4.3	4	0
	3	35	0	35	0 (0)	42.9 (15)	42.9	3.7	3	6.7 (1)
	4	35	0	35	5.7 (2)	20.0 (7)	25.7	6.6	6	0
	5	35	0	35	2.9 (1)	42.9 (15)	45.7	7.7	6	13.3 (2)
Canadian Review of Sociology and Anthropology	1	35	1	34	0 (0)	26.5 (9)	26.5	5.2	6	0
	2	35	1	34	0 (0)	26.5 (9)	26.5	6.7	7	0
	3	35	1	34	0 (0)	44.1 (15)	44.1	7.3	7	6.7 (1)
	4	35	0	35	2.9 (1)	54.3 (19)	57.1	6.4	6	5.3 (1)
	5	35	1	34	5.9 (2)	23.5 (8)	29.4	9.0	8.5	12.5 (1)
Sociological Quarterly	1	35	0	35	0 (0)	28.6 (10)	28.6	7.4	7.3	0
	2	35	0	35	0 (0)	40.0 (14)	40.0	8.1	7	14.3 (2)
	3	35	1	34	2.9 (1)	41.2 (14)	44.1	12.4	10	42.9 (6)
	4	35	1	34	2.9 (1)	29.4 (10)	32.4	8.8	8.5	30.0 (3)
	5	35	0	35	5.7 (2)	34.3 (12)	40.0	8.6	8	16.7 (2)
Sociology and Social Research	1	35	1	34	2.9 (1)	50.0 (17)	52.9	5.8	6	5.3 (1)
	2	35	0	35	2.9 (1)	37.1 (13)	40.0	7.4	5	15.4 (2)
	3	35	0	35	0 (0)	42.9 (15)	42.9	4.0	5	0
	4	35	2	33	0 (0)	51.5 (17)	51.5	7.4	6	5.9 (1)
	5	35	1	34	11.8 (4)	47.1 (16)	58.8	7.8	8	0
Pacific Sociological Review	1	35	1	34	2.9 (1)	50.0 (17)	52.9	6.2	6	0
	2	35	1	34	2.9 (1)	44.1 (15)	47.1	7.3	5	6.7 (1)
	3	35	3	32	3.1 (1)	50.0 (16)	53.1	7.4	7	12.5 (2)
	4	35	0	35	2.9 (1)	34.3 (12)	37.1	5.4	4.8	8.3 (1)
	5	35	0	35	8.6 (3)	34.3 (12)	42.9	7.9	5.5	8.3 (1)

is 2.9%. For median prestige, the within-journal variations tend to be greater than the between-journal variations as well. For percent rankings above ten, the within-journal variations again are greater (three have ranges of over 33%) than the between-journal range of half that size. Finally, the data on the mean prestige ratings of journals also reveal greater within-journal than between-journal variation (analysis of variance, N.S., table omitted).

In conclusion, highly regarded articles appear in less highly regarded journals (as defined by Glenn's sample), while less highly regarded articles appear in highly ranked journals. This finding would be pedestrian were it not for the fact that it is often forgotten. If individuals want to evaluate a piece of scholarly research, they would be well advised to read the actual article and, as the subject of this 6.2 research note suggests, not judge a book by its cover.

REFERENCES

American Sociological Association
  1974 Guide to Graduate Departments of Sociology, 1974. Washington, D.C.
Glenn, N. T.
  1971 "American sociologists' evaluations of sixty-three journals." American Sociologist 6:298–303.

EXCHANGE

A COMMENT ON DUSTER, MATZA & WELLMAN: PROTECTION OF HUMAN SUBJECTS IN FIELD RESEARCH*

Theodore N. Greenstein

Southern Illinois University

The Duster et al. (1979) paper raises some important questions regarding limitations placed on field researchers in the form of DHEW policies (specifically, Part 46 of Title 45 of the Code of Federal Regulations) concerning the protection of human subjects. Most social scientists would agree that field research strategies (including field experimentation as well as participant and non-participant observation) occupy an important role in our attempts to explain and predict human behavior, and that efforts by DHEW or any other government agency to bureaucratize field research out of existence should be met with strong opposition.

Unfortunately, the Duster et al. paper does little to enlighten us about the reality of any such threats by DHEW. In fact, it is difficult for even the concerned professional to deduce anything about DHEW policies regarding human subjects · protection in general, and policies concerning approval of field studies in particular, from their arguments. The major problems come from their casual use of the term "risk" and their misconceptions concerning the applicability of informed consent requirements.

Duster et al. allege that DHEW regulations state that "Any social, psychological, or financial harm which might result from research is considered to be a risk" (1979:139), despite the fact that Section 46.103(b) of DHEW regulations defines "subject at risk" as:

> . . . any individual who may be exposed to the possibility of injury, including physical, psychological, or social injury, as a consequence of participation as a subject in any research, development, or related activity which departs from the application of those established and accepted methods necessary to meet his needs, or which increases the ordinary risks of daily life, including the recognized risks inherent in a chosen occupation or field of service.

The operative phrase here is "which increases the ordinary risks of daily life." Relatively few sociological studies—field studies included—do anything to appreciably increase "the ordinary risks of daily life." Probably the

---

* Address all communications to: Theodore N. Greenstein, Department of Sociology, Southern Illinois University, Carbondale IL 62901.

greatest threat posed by the vast majority of sociological studies is the possible breach of confidentiality, a problem discussed at length in a recent article (Bond, 1978:144–152) and subsequent comments and reply.

That the study on discrimination (which the authors misrepresent as a participant observation study, when in fact it constitutes a field experiment due to its introduction of an independent variable) would have been "routinely" disapproved by DHEW or a local institutional review board (IRB) is not nearly so obvious as Duster et al. would have us believe. Returning to the DHEW definition of "subject at risk," we see that the "recognized risks inherent in a chosen occupation or field of service" are *not* considered as part of the risk calculus. It is certainly within the realm of possibility that DHEW or an IRB might well consider the risks presented by the study and its findings (suitably presented in terms of aggregate data with regard for confidentiality) to be within the "recognized risks" for persons working in the real estate profession.

Another misleading statement by the authors argues that DHEW policy on human subjects protection "is a blanket one which does not recognize variation in research problems, and therefore in research methods" (1979:139). But Section 46.113 outlines circumstances under which a "short form" written consent document, or no written document at all, may be acceptable.

One common misconception held by many sociologists is that researchers must routinely obtain signed statements of informed consent. DHEW regulations at Section 46.113(b) specifically permit the IRB to "waive the requirement for the researcher to obtain documentation of consent" under specified conditions. One of these conditions is that "the research presents no more than minimal risk of harm to subjects and involves no procedures for which written consent is normally required outside of the research context." Alternatively, the requirement of informed consent may be waived when the existence of a signed consent document would be "the only record linking the subject and the research" and that "the only significant risk would be potential harm resulting from breach of confidentiality." Given these alternatives and options available to the IRB, it seems that researchers could justifiably petition for waiving the informed consent requirement for many kinds of field studies, including the one discussed by Duster et al.

It should be clear that Part 46 of Title 45 CFR does not constitute a prohibition of participant

observation studies or of any other mode of research. On the contrary, DHEW is in the process of soliciting comments on a proposed amendment to these regulations that would exempt "research involving solely the observation (including observation by participants) of public behavior" from the human subject review process when "the subjects cannot be reasonably identified, directly or through identifiers linked to the subjects" (Code of Federal Regulations, 1979:47593).

The protection of human subjects is a difficult and complex task, and I appreciate and share the concerns of my colleagues who fear unwarranted bureaucratic limitation of legitimate research activities. However, the importance of these issues for the discipline requires a careful examination of the facts and acquaintance with DHEW and IRB procedures on a first-hand basis. I would encourage my colleagues to show their concern for these issues by becoming involved in the operations of their respective IRBs.

REFERENCES

Bond, Kathleen
    1978 "Confidentiality and the protection of human subjects in social science research: A report on recent developments." The American Sociologist 13:144–152.
Code of Federal Regulations
    1979 Protection of Human Subjects. Federal Register 44:47692–47698.
Duster, Troy, David Matza, and David Wellman
    1979 "Field work and the protection of human subjects." The American Sociologist 14:136–142.

REJOINDER

Interpretations, Regulations, and Facts*

If one were to read only Greenstein's commentary on our original article, the conclusion might be that we disagree about facts. On the contrary, the differences are primarily over interpretation. Let us take up his three major points. First, he says that our use of the term "risk" is too "casual" and this allows us to misinterpret the DHEW guidelines too broadly. But his selection of a quotation from our paper covers his own objection. Namely, he correctly quotes the paper by saying that "Any social, psychological, or financial harm which might result from research is considered to be a risk." And then, as if there were disagreement, he quotes Section 46.103(b) of the DHEW regulations with the introduction: "*de-*

spite the fact." Despite what fact? Could anyone demonstrate that the phrase "might result from their research" is in any way substantively, practically, or theoretically different from the phrase ". . . which increases the ordinary risks of daily life?" Quite simply, if such harm might result from the research, and the research was not a feature of ordinary daily life, then the research is covered by the language of risk.

He then says that relatively few sociological studies do anything to appreciably increase the ordinary risks of daily life.

He may well be correct. But to the extent that sociological studies focus upon the different interests of groups socially stratified (an issue we think is an important sociological research enterprise), those groups may not agree with him in his *interpretation,* nor may an institutional review board (IRB), nor may a DHEW review panel. And the possible effect of such disagreement could be a veto or restructuring of research interpreted by *these* groups as increasing the normal risk of daily life. Greenstein's interpretation of risk is not as germane as the one made by people with the power to enforce their interpretation, and this very exchange demonstrates variable reading of the language in Section 46.103(b). (Does anyone care to define "social injury?")

His second point is that the study of housing discrimination we cite might well have been approved by an IRB or DHEW. He says that the study might have been approved had the findings been "suitably presented in terms of aggregate data with regard to confidentiality." The issue, however, does not concern the presentation of findings. It relates rather, to the collection of primary data. Every IRB that we know about (one of us sits on an initial review group at DHEW; another sits on a departmental Human Subjects Committee) demands, *routinely,* that subjects of research be informed that they are the subjects of research. If Greenstein knows of an IRB that does not make such demands, he should let that be known. We take the position that this requirement is, routinely, a good policy. However, in a housing discrimination study, such a policy would render the study useless for reasons we discussed in the article. Greenstein interprets the guidelines to permit this kind of research, *without informed consent.* We strongly suspect that he is wrong. We leave the matter to empirical investigation, for it is an empirical question. However, as will be clear from the next point, the empirical case against his view is likely to be overwhelming.

The third point he makes is that DHEW guidelines are quite flexible on the matter of

informed consent; about when it may be waived, and when a short form is acceptable. But Greenstein himself quotes Section 46.113(b) which says that there are circumstances under which one may waive written *documentation* of consent. That is quite a big difference from the requirement that the researchers inform subjects verbally of their research and obtain verbal consent to proceed. In the housing discrimination study to which we referred, this requirement might be executed in the following manner and with pre-·dictable results. "Good afternoon. I am here doing a study of discrimination against blacks in housing. As you can see I am black. Do you discriminate? . . . etc."

And Greenstein says that *we* are being "casual."

Greenstein, in a parenthetical gratuitous remark, accuses us of "misrepresenting" the housing discrimination study as a participation observational study, "when in fact it constitutes a field experiment due to its introduction of an independent variable." What fact? How many methods textbooks concur with his position that the term participant observation does not cover research in the natural setting, where researchers participate in and observe the world and then pair off experiences for comparison?

Greenstein concludes that DHEW guidelines do not prohibit field research. We agree. We did not discuss prohibition; we addressed the issue of degree of difficulty, and degree of rendering less meaningful.

Greenstein ends his commentary with advice that colleagues show concern for these issues by becoming involved in the operations of IRBs, heavily implying that our criticism is theoretical and without experience. One of us is now, and has been for five of the last seven years, a member of a review panel for DHEW research grants, and for the last decade, director of a graduate *field research* training program where graduate students must submit detailed protocols, including informed consent requirements, to the local IRB. Another has been a member of a committee that reviews student and faculty research proposals for their treatment of human subjects. Those are facts.

Troy Duster
David Matza
David Wellman

* Address all communication to: Troy Duster, Department of Sociology, University of California, Berkeley, CA 94720.

COMMENT

A COMMENT ON ST. GEORGE AND McNAMARA: "'FILTHY PICTURES' OR THE CASE OF THE FRAUDULENT SOCIAL SCIENTIST"*

James W. Loewen

Catholic University and University of Vermont

As a sociologist who has testified fairly widely in court, I was fascinated by the anecdote of the fraudulent expert described by Arthur St. George and Patrick McNamara (1979). The anecdote does not support their policy recommendations, however.

They faced a social scientist who largely fabricated and wholly misrepresented his "data." From this experience they conclude that the American Sociological Association should certify a subset of our membership as "expert" in given fields and should begin "by establishing a registry of individuals competent to conduct sociological research" (1979:148). This remedy would not have prevented the case of fraud they faced, but would add only another cumbersome certification level to our profession. Three other remedies are available which entail no bureaucratic cost.

The fraudulent expert was in fact stopped in this instance, stopped by the intelligent actions of the U.S. attorneys who faced him. Generally this is the best line of defense against fraudulent expertise in the courtroom, or against the far more common problem of the inappropriate use of legitimate expertise. Opposing attorneys have a direct interest in stopping such practices. As sociologists we need to increase the sociological awareness of the legal profession so that lawyers are less in awe of statistics and research findings, more able to criticize or at least to question them. This is my first recommendation to the profession (and toward this end I will be devoting the autumn issue of the *Clearinghouse for Civil Rights Research* to presentations of some specific ways that statistics and social research have impinged on judicial proceedings).

This is a realistic recommendation. Not only did it succeed in the case presented by St. George and McNamara, it has worked increasingly well in school desegregation litigation and other areas as lawyers grow more sophisticated about sociology. Recently I observed a deposition at which a crucial error

committed by the "other side's" expert was uncovered not by me but by an attorney for "our side," who did not know precisely what he was questioning, but refused to be cowed by his own ignorance. By the time the expert explained her calculations to his satisfaction, her error had become apparent to all, including the expert herself. Recalculation led to a quite different conclusion.

A second recommendation is that we involve ourselves more fully in the litigation process and the business of expertise. We should encourage attorneys to contact us, without fee in minor consultations, in order to iron out questions about research they cannot comprehend, and to subject expert conclusions to peer scrutiny. The reason expertise is hired in the first place is because questions about the social structure *are* involved. We must be careful in what we claim for our profession, but if we have *no* expertise in this area, then our whole field is a fraud, in or out of court.

Again, this recommendation is realistic: social scientists increasingly face each other on each side of the aisle in court. Although this raises serious questions of "conclusions for hire," it does make some elements of the scientific review process available to the court.

Now consider for a moment the certification process advanced by St. George and McNamara. They use psychology as a model. Presently the American Psychological Association does certify experts. With what result? Every attorney knows that s/he can find a psychiatrist or psychologist, somewhere, to say just about anything—that a person is or is not sane, that a child belongs with its father, that a child belongs in its mother's care, etc. "Hired gun" is the term often used by lawyers. Certification has not increased the repute of psychologists and psychiatrists among judges and attorneys to any level above that of sociologists, so far as I can ascertain. Instead, at least when the process works at its best, the logic and conclusions of each expert are examined carefully by the attorneys and judge, in line with my first two recommendations; that is as it should be.

Would certification have deterred H.? He already *is* a psychologist. Much is made of his Ed.D. in educational psychology as if that were decisively inferior to a Ph.D. in social psychology. But with either degree, he would never have been touched, positively or negatively, by any ASA certification process. Certification could have a chilling effect on the appropriate

use of sociologists as experts. Many sociologists are now expert in areas which they never studied or which never existed when they attended graduate school. Some sociologists, including at least one recent President of the ASA, do not even hold the doctorate. They would be excluded. Moreover, there are far fewer sociologists than psychologists in America. Hence many parts of the United States would be wholly without certified experts, at least in many subfields. Affluent defendants, such as governmental bodies accused of discrimination in civil rights lawsuits, could afford to fly in the most highly certified expert from afar; plaintiffs, on the other hand, would be denied access to expertise since no social scientist in their area was certified.

What *would* have deterred H.? What would deter him from continuing to purvey fraudulent expertise? The consequences must inure to him personally. And that is my final recommendation. St. George and McNamara "regret our own failure to make a complaint about H's performance to any professional organizations to which he may have belonged, or to his employing institution" (1979:145). Why regret this in *TAS*? Why not make the complaint? The incident is hardly ancient history!

It is difficult, maybe even dangerous, to take matters like this into our own hands, but whose hands are better suited for the job? If a colleague fails to meet class after class, students will complain to us. It is then our duty to meet with the professor, perhaps pass the information to our departmental chairperson, or otherwise act to protect students from abuse. We would not get the ASA to pass an ethical code stating that sociologists should not cut class. Similarly, we must confront and conflict with the unethical expert. It's messy, it's serious, but then so is the situation. No other remedy will deal with the problem.

REFERENCES

St. George, Arthur and Patrick H. McNamara
  1979   "'Filthy Pictures' or the case of the fraudulent social scientist: Unmasking the phony expert." The American Sociologist 14:142–149.

JHSB publishes reports of empirical studies, theoretical analyses, and synthesizing reviews that employ a sociological perspective to clarify aspects of social life bearing on human health and illness, both physical and mental. Its scope includes studies of the organizations, institutions, and occupations devoted to health services, as well as studies of the behavior of actual and potential recipients of these services.

Recent issues contain reports on:

Social Class and the Use of Professional Help for Personal Problems: 1957 and 1976, RICHARD A. KULKA, JOSEPH VEROFF & ELIZABETH DOUVAN

Health Occupations in Israel: Comparative Patterns of Change During Socialization, JUDITH T. SHUVAL AND ISRAEL ADLER

Occupational Stress and Health Among Factory Workers, JAMES S. HOUSE, ANTHONY J. McMICHAEL, JAMES A. WELLS, BERTON H. KAPLAN, & LAWRENCE R. LANDERMAN

The Functional Importance of Sex Roles and Sex-Related Behavior in Societal Reactions to Residual Deviants, WILLIAM A. RUSHING

Stress, Social Status, and Psychological Distress, RONALD C. KESSLER

THE SOCIOLOGY OF

KNOWLEDGE IN AMERICA

1883-1915

Ellsworth R. Fuhrman

Ellsworth Fuhrman analyzes the sociology of knowledge in the works of the first generation of American sociologists. He provides a unique history of the early years of sociology in this country by focusing on six prominent scholars—William G. Sumner, Lester F. Ward, Franklin H. Giddings, Albion W. Small, Edward A. Ross, and Charles H. Cooley. He scrutinizes their attitudes about the relationship between social structures and the production, distribution, and consumption of ideas, while providing considerable biographical information about each. Fuhrman argues that there are two distinct political traditons ·within the sociology of knowledge—the social-technological and the critical-emancipatory—and that the early American sociologists markedly favored the former. He further analyzes the intellectual biases of these sociologists.

6 x 9. 288 pp., index. $20.00.

Continued from Cover 2

there is basis for hope—for the few, in the form of a sustained analysis of their work—for the many, at least a citation. That is a slightly comforting thought as the gray edges of winter recede and the crocuses pop through in their perennially surprising way.

The issue is completed by a paper by Heilman describing the potential estrangement of doing field work as a native, and a self-designated 6.2 paper by Teevan on the variable quality of articles published in sociological journals. In the latter instance, we have lifted, ever so slightly, the previous editor's moratorium on papers that examine the publication practices of the profession. Whether we publish more papers in this genre will depend very much on the quality of the issues they raise for the profession.

Themes popping up, crocuses popping up—it is enough to make the reader wonder if the editor hasn't been confined too much to the office this winter. Perhaps there is hope for more immortal words in the editor's note for the Summer issue.

THE ASA AT 75

SHOOT THE PIANO PLAYER?—
S/HE'S DOING THE BEST S/HE CAN!!
(or is s/he?)

The American Sociological Association has survived for 75 years as an association formed of, and for, persons sharing "sociological interests." It has attempted (wisely and well/ unwisely and poorly) to represent sociology as a discipline and profession to others. Our 75th year is a propitious time to assess what the Association has become and what it may have failed to become. How well does it meet our various objectives, issues, and concerns? *The American Sociologist* seeks papers that:

- provide insight into the development of ASA as we now know it

- assess particular ways that ASA (responds/does not respond) to professional and disciplinary needs

- analyze ASA's relationship to other associations and institutions (social, educational, governmental)

- offer proposals for changing ASA's mode of operations and activities

- critique ASA's role with respect to changes in American and other societies.

∽

DEADLINE FOR SUBMISSION OF PAPERS: *September 15, 1980,*
CONTACT: James L. McCartney, Editor/*The American Sociologist*
Department of Sociology / University of Missouri / Columbia MO 65211.
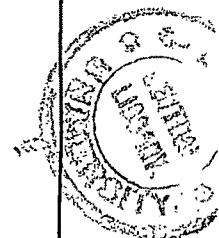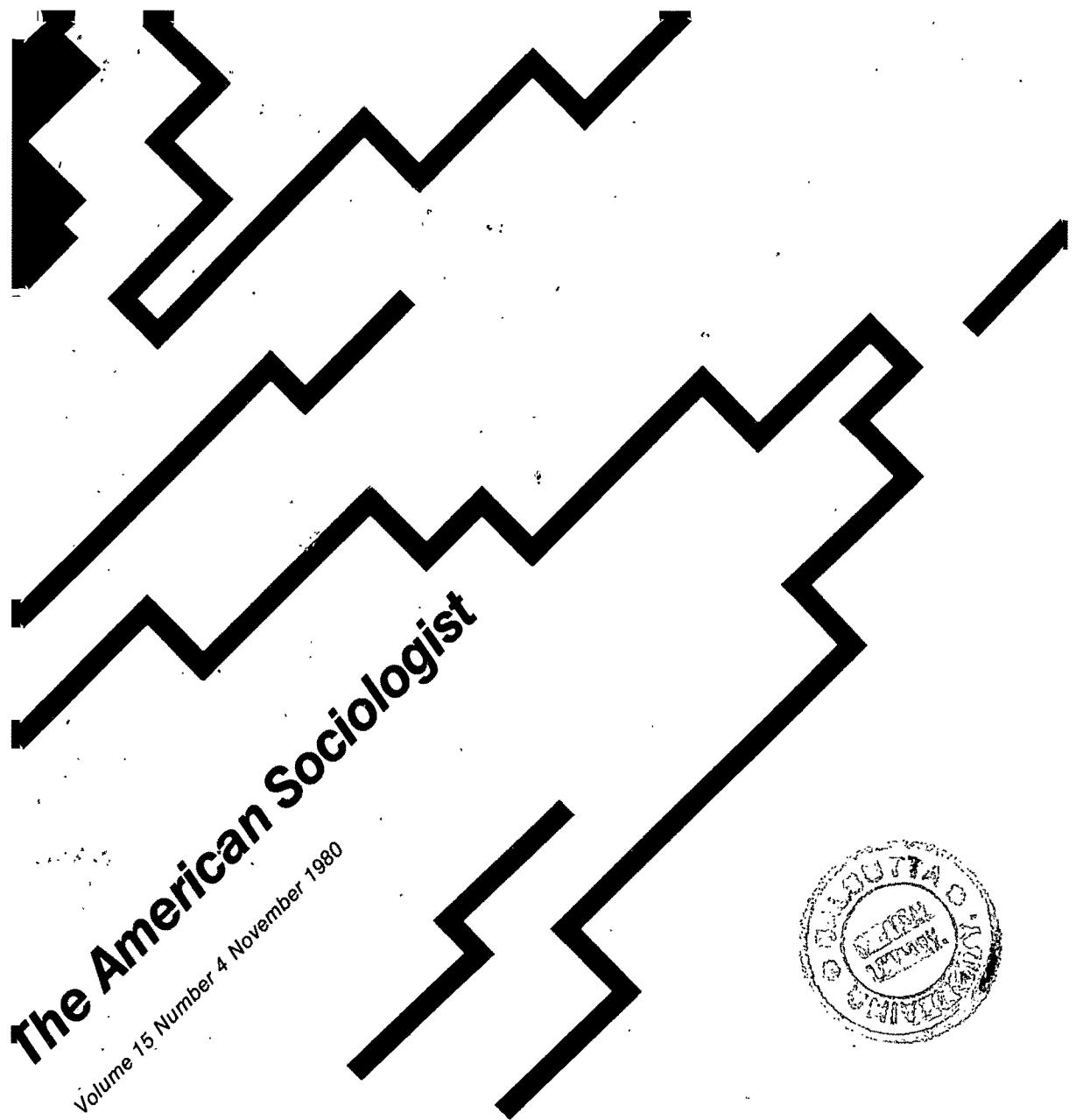
The American Sociologist

Volume 15 Number 4 November 1980

EDITOR'S PAGE: Peer Review

GUEST EDITORIAL: HELEN MACGILL HUGHES*

The process of review by peers is an elaborate ritual in which editors, referees, and the contributing authors play their parts, the outcome being, it is hoped, the identifying of the best articles among those offered. The parties to the ritual are presumed to be each others' peers, and while the acceptability of a given article may be questioned, no one challenges the editorial right to pass judgment.

Reviewing on the journals may be by the "open" method, by which authors and referees are identified by name, or by the "blind" or anonymous method, which many editors, perhaps most, prefer. There are several forms of blindness: the editor removes the author's name and other identifiers from the manuscript, so that the reviewers do not know whose paper they are judging; or the reviewers' identity is hidden from the author and they may feel free to criticize the paper under the cloak of anonymity. Blindness is "double" on *Human Organization* [and *TAS*], where authors are unknown to referees and referees are unknown to authors. *Publications of the Modern Language Association of America (PMLA)* follows a pattern of blindness in which the first level of readers may, if they choose, keep their names hidden from the author, but the second level identify themselves and sign their opinions.

In 1956 the *American Sociological Review,* after 24 years of the open system, announced that reviewing from henceforth would be blind, authors being unidentified. No reason was given, nor does anyone know, as far as I can learn, what precipitated the change. A few

years later, the *American Journal of Sociology* followed suit. All the journals of the American Sociological Association are read blind, as are those of the anthropologists. And yet, not until January of this year did the 96-year-old *PMLA* adopt the blind system. On the other hand, the prestigious *Journal of the American Medical Association,* which describes itself as a professional peer-review publication, makes no attempt to conceal authorship. In the face of these contradictions, it is ironical that, twelve years ago, Diana Crane (1967:197) demonstrated that anonymity does not significantly reduce bias in reviewing.

It must be admitted that anonymity is easily breached. Particularly in psychology, a field reputed to have the best organized and most envied network of peers, appearances may be solemnly preserved, even when anonymity is a farcical illusion. The bibliography and citations can be counted upon to betray "the invisible college" in which the author's network belongs, to anyone who knows the field. Who can tell when anonymity is intact? And who knows when scholars play little "pretend" games with each other?

Champions of blindness and of openness alike cherish strong convictions that they know which is the best way to be sure good papers are published and bad ones rejected. A referee complained, "I need to know if the author is in mid-career, or is this a youthful first effort; and I want to know who he or she is." And an indignant European sociologist wrote to the editor, "How could those reviewers recommend rejection of my paper? Didn't they know I was the author?" The editor of an anthropological journal, declaring openness is best, added, "I would accept ethnographical data from experienced researchers which I

The American Sociologist

EDITOR'S PAGE   Inside Front Cover

For information for contributors, see TAS 15(1, February), inside back cover.

◆

ONE YEAR AFTER THE RESTORATION OF SOCIOLOGY IN CHINA

WANG KANG*

*The Chinese Academy of Social Sciences
Peoples Republic of China*

WITH AN INTRODUCTION BY

MARTIN KING WHYTE**
University of Michigan

The recent restoration of the field of sociology in China, and the renewed contacts with the outside world that Wang Kang's article symbolizes, are welcome events for American sociologists. As Wang discusses, the discipline was proscribed in 1952, and in 1957 an effort to revive the field failed, with many leading Chinese sociologists branded as "rightists" and cast into political oblivion. Much the same fate befell sociology in the Soviet Union in the 1930s, and for much the same reason: Sociology as a science of society was seen as a threat to the officially approved science of society, Marxism-Leninism. In the case of China we are fortunate that many of the earlier practitioners of our trade survived and kept hoping the day would come when the field could be revived. With the political changes that occurred in China after 1976 conditions finally became suitable, and in the spring of 1979 an officially approved revival of Chinese sociology began.

Wang Kang is one of the leading figures in this revival effort. A protegé and colleague of Fei Xiaotong, Wang remained intellectually active during the years of the ban on sociology, publishing a biography of Wen Yiduo, one of modern China's intellectual leaders, who was assassinated in 1946. Wang now serves as secretary-general of the Chinese Sociological Research Association (CSRA), the counterpart to our ASA, and is playing a key role in establishing an Institute of Sociology within the Chinese Academy of Social Sciences.

As Wang's article makes clear, reestablishing sociology after almost thirty years is no easy task. The first step was simply to win official support for sociology, and to convince critics that the discipline poses no threat to the Chinese system. As the article makes clear, this involves arguing that sociology can be compatible with Marxist doctrine, rather than competing with it. Wang is quite frank in stating that sociology within China will therefore have to develop in distinctive and applied ways, as a tool to help the authorities deal with various kinds of social problems. The perception within the leadership that social problems exist in China for which official doctrines provide no ready solutions is clearly one of the reasons why the sociological revival has been permitted. In the CSRA meeting in the spring of this year it was decided to focus particular attention on how to deal with the problems of youths, particularly on alienation and juvenile delinquency.

This applied and social problems focus should not be surprising, since it is much the same kind of "bargain" reached to allow the revival of sociology in the USSR in the

* Mr. Wang Kang, Deputy Director; Institute of Sociology; The Chinese Academy of Social Sciences; 5,Jianguomen Nei Da Jie; Beijing, China.

** Martin King Whyte, Department of Sociology, University of Michigan, Ann Arbor MI 48109.

early 1960s. The constraints this particular mission places sociology under mean that our Chinese colleagues will have to be selective in their borrowing of ideas and techniques from the West and from other countries. For the moment, at least, we cannot anticipate large numbers of students from China coming to enroll in our doctoral programs, since simply catching up with advanced work abroad is seen as less suitable in sociology than it is in the physical sciences. Nevertheless, Chinese sociologists are eager to reestablish contacts overseas and learn about developments in the discipline internationally. Here they are understandably having some difficulty trying to sort through the ways the field has developed in their absence. Fei Xiaotong, when he travelled to America earlier this year, described himself as a Chinese Rip van Winkle, and some of the same groping to come to terms with the diversity and confusion in our field can be seen in Wang's article.

As Wang makes clear, he and his colleagues are only at the beginning of their effort. The first university department in sociology is only being set up and enrolling its first students this fall, and there are great shortages of books, research facilities, and financial resources, as well as of trained personnel. At the same time high expectations are being raised for what sociology can accomplish in dealing with the problems of Chinese society. It will be interesting to see in the coming years how our Chinese colleagues meet the demanding challenges facing them.   MARTIN KING WHYTE

It has been a year since sociology was restarted in China. The establishment of the Chinese Sociological Research Association (CSRA) marked the beginning of the restoration and was proposed during the Discussion Meeting on Sociology convened by the preparatory committee of the National Philosophy and Social Science Planning Conference which was held in Beijing from March 15 to March 19, 1979.

CSRA is the first academic association in sociology established after the discipline was eliminated almost 30 years ago. It is composed of professional and amateur sociological researchers. Its mission is to serve the Four Modernizations[1] in our country by conducting sociological research and studies on social problems under the guidance of Marxism-Leninism and the thought of Mao Zedong.

Professor Fei Xiaotong (Fei Hsiao-tung), a well known sociologist and anthropologist, serves as the president of CSRA; Professors Chen Dao, Lei Jieqiong

and five other professors serve as vice-presidents; some 20 senior sociologists and well known scholars from other fields who support sociological research, among them Yu Guangyuan, Chen Hansheng, Wu Wencao, and Wu Zelin, serve as advisors; and more than 40 others serve as its standing members. The CSRA also has reserved seats for sociologists in our Taiwan province and will welcome them to come back to our country to work together with our scholars to discuss and study sociological theories and concrete social problems.

Following the principle of "let a hundred flowers bloom and a hundred schools of thought contend,"[2] set by the Chinese Communist Party, the CSRA has in the past year frequently organized a variety of academic activities. Together with government departments, associations, and individuals, the CSRA has started to conduct social investigations on some social problems. It also has held a series of thematic talks and lectures, translated some works, and published some reference materials.

The restoration of Chinese sociology also has caught the attention of foreign

---

[1] [This and subsequent footnotes were prepared by Martin King Whyte.] The "Four Modernizations" phrase refers to the official development program of China's current leadership, and was a slogan first used by the late Premier Zhou Enlai in 1975. The specific modernizations referred to are agriculture, industry, national defense, and science and technology.

[2] This "hundred flowers" slogan was used in 1957. It is being used again in China now to emphasize increased tolerance of diverse views and practices.

sociologists and international academic associations. International exchanges have become more frequent and this has promoted friendship between scholars as well as the peoples of China and of other countries; sociologists of our country have been invited to visit more than ten countries on the European, American, and Australian continents; they have received visiting sociologists, anthropologists, demographers, and philosophers from many countries; and have held many seminars and exchanged ideas. The CSRA also has organized talks about the achievements, trends, and activities of sociology in other countries. Some foreign scholars and academic associations have · sent us academic journals and books on sociology and other related topics.

After it crushed the "Gang of Four," our government has been concerned very much with the restoration of sociology. Hu Qiaomu, the Director of the Chinese Academy of Social Sciences, in his important speech given at the Discussion Meeting on Sociology, clearly restored the good name of sociology, and he encouraged sociologists to actively and industriously conduct sociological research. He also pointed out that there are some things correct and some things incorrect in modern sociology. Whether an idea is self-proclaimed Marxism or accused of Marxism, it must be treated with a scientific attitude. If it has been proven by practice as an objective truth, it is an objective truth; if it has not been proven an objective truth, it remains a hypothesis and must be treated as a hypothesis. We must adopt analytic methods and develop a sober and healthy attitude toward perspectives, theories, methodologies, and knowledge in modern sociology. We must adopt this critical attitude toward research done by anyone, even toward our own. Only by doing that can we make progress.

Vice-premier Yao Yilin also pointed out, when he received some American sociologists,[3] that we must focus on the actual situation of our society to conduct sociological research and promote international academic exchanges.

_____

[3] C. K. Yang and a delegation from the University of Pittsburgh.

In the past year, as evident in newspapers, magazines, and journals, sociology already has won support from every sphere of our society, and sociological research has been initiated, thereby completing the necessary preparation for setting up a sociological research institute in the Chinese Academy of Social Science. Moreover, regional sociological associations and teaching institutions have been either established (for example, Shanghai) or are under preparation. However, the discipline now is facing the problem of being short of talent because it was eliminated during the reorganization of college departments and the curriculum of universities in 1952 and heavily attacked again in 1957.

For nearly 30 years, sociology has been regarded as a prohibited area, and all relevant activity ceased. The discipline has not been able to train new professionals. Furthermore, those who were trained and engaged in teaching and research in sociology some 30 years ago are now aged, and their professional knowledge is limited to a very narrow area. Thus, a very urgent task in the restoration of sociology is to nurture new-born forces (talents). We hope that teachers and researchers in philosophy, history, or other social science disciplines and comrades who work in relevant professional institutions and have practical experience will support sociology and social investigation on social problems, and will work together with us to make great efforts in rebuilding sociology.

When we say "restoration," we use the term to contrast the situation in the past where sociology was eliminated and activity ceased for a long period of time. It certainly is impossible to restore the sociology of the past without making changes, since new China has undergone drastic changes in the past 30 years; it also is impossible to indiscriminately copy foreign sociology. The correct way is to focus on the actual situation of new China and follow the guiding ideas set down by the constitution of our country—the principles of Marxism (historical materialism and related concepts)—and then critically adopt some useful perspectives and methods from classical, contemporary,

and foreign sociology to rebuild a Chinese sociology which serves socialism in our country.

In the past year, through discussion, the sociological circles in China have clarified several theoretical issues. An assertion, which was fashionable and dominant when sociology was abandoned, held that sociology is not needed since we have historical materialism. We must say that historical materialism is indeed a guiding idea and it can help us to study all sciences concerned with society; however, it cannot substitute for them. This is because historical materialism is not equal to all the subject matter of the social sciences, nor to that of sociology. It is certainly wrong to think that the problem of sociology can be solved and sociology can be eliminated when we have historical materialism. For instance, historical materialism puts the emphasis on the study of the forces and the relations of production. Although this does provide a key for understanding social life, it is not possible to cover all aspects of social life, because the relations of production, though the core of social relations, do not cover all patterns of social relations, just as social life consists of more than economic life.

There was still another point of view which claims that there should be no social problems in socialist countries since classes and exploitation have been eliminated, and that to say they exist is equal to smearing socialism. However, practices have already proven this assertion invalid. It is evident that all countries have social problems, and so do the socialist countries, even though their nature and methods of solution are different. Contradictions exist universally. The solution of contradictions and social problems depends on the joint efforts of all kinds of social sciences and other disciplines. Certainly, sociology is expected to contribute its share.

Although sociological research has just been resumed, interesting issues have already caught our attention. We are aware that, from what we learned in the past, there is no consensus on the definition of sociology and that the subject matters covered are complicated in content and of great range. After having come into renewed contact with foreign sociology, we have also noticed that, due to the changes in contemporary society and in science and technology, the analysis and research functions of sociology have expanded, so that now the field not only has relations with other social sciences, but also has been influenced by knowledge from many areas in the natural sciences. As the result of extensive use of statistics and computers, quantitative analysis has increasingly been used in areas where only qualitative analysis was used in the past. The development of modern behavioral science and systems engineering has made the relation between all of the social sciences—but particularly sociology—and the natural sciences closer than ever. These developments are a topic of great interest to Chinese sociologists.

After sociology was again put on the research agenda, another new reaction developed. Many people seemed to think all other disciplines could be placed within the boundary of sociology, and they feel that sociology is an all-encompassing, extremely broad discipline without a well delineated boundary. As a newly restarted academic discipline with a weak foundation, sociology should respect and cooperate closely with other academic disciplines; it should focus on areas untouched by other disciplines or residual spots among the disciplines. Otherwise, the sphere of sociology would become too broad and the main areas for research in the field would be hard to specify.

This impreciseness has existed in sociology for quite some time and people's reactions to it are quite different. Some consider this as just an indication of the immaturity of sociology. Some others feel that this is because society is so complicated, patterns of change are multifaceted, and any social phenomenon is affected by many kinds of influences. Since scholars look at things from different angles, they certainly will arrive at different conceptions, and this can help us observe and understand society from all angles. Moreover, this diversity also is because of the fact that people in a class society are conditioned by their class standpoint and emotions, and their reflections on events are necessarily different.

All these differences are reflected in academic research, and become the basis for forming different schools of thought which provide many different explanations of society and social life. Here it occurs to me that the reason why sociology is now relatively prosperous in many countries is probably due to its capability for providing effective formulas for maintaining social stability. Besides, it can be said that it is just because of the non-uniformity of sociology that sociologists can look into and analyze society and social problems in many countries from various aspects, angles, and factors; to explore the laws of change and development of a country or even society of the whole human kind; and to cooperate with other relevant academic disciplines in a mutually complementary way which helps each to expand its own research sphere. No doubt some of the theories and perspectives are correct and some are not. Some sound reasonable and some are forced interpretations, some have drawbacks in conceptualization, and some show class bias, etc. In short, these theories may contain only partial truths, but they are not totally fallacious. Also, the data quoted or cited as proof or evidence by each school may well be inevitably biased. However, if they are used just as reference material, they possibly may be used to induce our contemplation even though they may not add up to a complete picture. This is perhaps just our hope for this newly restored academic discipline.

Thus it can be seen that sociology should be of some use because it is a science which studies society, social life, social relations, and social problems. Modern and contemporary Chinese society has undergone drastic and rapid change and this makes the social life and social relations very complicated and subtle, and social problems highly varied. As can be seen from the current situation, there are old social problems which were inherited from the old society, and new problems which either arose in the new society or resulted from foreign influences. These need to be seriously and carefully studied.

Mainly, sociology applies an on-the-spot investigation method to observe, understand, analyze, and study the vari-

ety of social phenomena, social life, social relations, and all kinds of social problems. It also does comparative studies either horizontally (comparing contemporary societies or different countries) or vertically (comparing societies of different periods in history) by using a method of dissecting. It then draws on this to do theoretical inquiries about the pattern of society, social change, social structure, and other problems, thereby searching for the law and dynamics of social change and development and the origins of human society as well as the structuring and evolution of classes and varied kinds of social organization. Also, sociology is concerned very much with social investigations on concrete social problems for the purpose of providing formulas and measures to maintain the stability of the social order. Here, we need to point out that the content of sociology is conditioned by and for the service of certain classes and that sociology is needed by every government because it can perform the function of maintaining social stability. It is quite evident that the primary concern of every ruling class or ruling party is the maintenance of the stabilized social order. Only under such a condition can it consolidate and strengthen its domination. Only antagonistic classes and enemy forces attempt to stir up trouble. The ruling party itself will never do such foolish things that are harmful to its dominance.

It is just for meeting the need of socialist modernization that our country currently is conducting sociological research. The third meeting of the standing committee of CSRA, which was held in Beijing from February 29 to March 1, 1980, pointed out that in order to remedy the wounds of ten years of long catastrophe caused by Lin Biao and the "Gang of Four," we must, from the sociological perspective, have correct social policy for restoring and cultivating good social customs, and for eliminating social factors harmful to the realization of the Four Modernizations. Therefore, sociologists must meet the urgent needs of the people and make great efforts to conduct social investigations, and to study existing and emerging social problems. It is expected that the works of sociologists may be able to contribute not

only to the accumulation of scientific knowledge of society and of the ways social life is arranged, but also to provide relevant government departments with reference materials and scientific proof for developing correct policies to solve social problems.

Judging from the current social condition of our country, many problems—some are practical social problems and some are theoretical ones—deserve attention. However, to emphasize the urgency of the study on practical problems does not mean theoretical research can be ignored or abandoned. The standing committee of the CSRA seriously analyzed some contemporary social problems, such as those involving population, family, marriage, morality and customs, crime, youth education, the support of the aged, employment, housing, the relations between cadres and masses, and other abnormal phenomena, and noticed the fact that these problems have already influenced social life and social order. It is further pointed out that as the process of the Four Modernizations proceeds and the contact with the outside world increases, social problems of various types and intensities will continue to emerge (some have already occurred) in economic construction, material production, culture, science, technology, ideology, emotional life, and other aspects of our society.

At present, one of the important tasks of the ongoing reconstruction of sociology must be to realistically map out a relatively practical and feasible long term research plan to set a foundation for future research based on the social reality of our country. Currently, the CSRA will work together with some already established regional sociological research associations to strengthen the unity of the sociological community to organize various kinds of academic discussions, and to gradually expand sociological teaching teams to teach and diffuse knowledge and methods of sociology, and to train new talents for sociology. Also, CSRA will act in response to the development of the international situation, to promote international academic exchange, and to understand the trend and development of sociology out-

side China, and to obtain support and assistance from the outside world, taking circumstances into consideration.

According to the proposal of the standing committee of CSRA, professional or amateur sociologists may, depending on their own situations, select an important area of social life and social problems to conduct investigations and research on, either individually or in a group; they can go to a factory, an agricultural production team, a commercial shop, a school, or a street to carry on relatively extended and continual comparative observation and analysis.

To Chinese sociology, the achievement of these tasks is not easy since it is new and its foundation still is very weak. However, the Chinese sociologists are still full of confidence because our country has already thoroughly passed out of a shaky and unstable period, and has steadily started a new march into a new period of stability and construction; ours is a united as well as a highly organized society, so that we can expect the cooperation and support from every organization; and the unanimity between investigated and investigators is an advantage and provides convenience for investigation and study. Besides, we also can follow the guiding ideology set down by our constitution, we can sum up historical experiences and lessons, and maintain a sober scientific attitude to make sociology practically contribute to the Four Modernizations.

Finally, we also want to happily acknowledge that we have received assistance from sociologists from many countries (certainly American sociologists are included). Here, please allow me to convey my sincere wishes to American sociologists and the American people. May the academic exchange and friendship between the sociologists and peoples of our two countries grow and prosper.

(Editor's note: Interested readers may wish to read a recent assessment, "Sociology and Anthropology," by Martin King Whyte and Burton Pasternak, in *Social Science Research in China*, edited by Anne F. Thurston and Jason H. Parker. Social Science Research Council, 1980.)

SOCIOLOGY IN JAPAN AND *SHAKAI-ISHIKIRON**

TOSHIO YAMAGISHI AND MARY C. BRINTON

University of Washington

*The development of Japanese sociology in the twentieth century is reviewed, with particular emphasis on the roles played by the influence of Western theories and the contemporary sociopolitical climate in Japan. By focusing on a subfield unique to Japanese sociology—*shakai-ishikiron, *or the "study of social consciousness"—we are able to view in microcosm the influence of Japanese industrialization and rapidly changing socio-political circumstances on the development of Japanese sociology as a whole. The multiple intellectual origins of* shakai-ishikiron, *including both the European and the American variants of the sociology of knowledge, Marxism, neo-Freudian psychology, and psychometric methods from American psychology, are discussed. The divergent development of theory and methodology within* shakai-ishikiron *is examined, and comparisons are made to trends in the sociology of knowledge in Europe and the United States.*

INTRODUCTION

If one were so inclined, one could probably write a history of the social sciences in Japan in terms of successive waves of Western influence. The academic traditions of Germany and the United States have had a particularly strong impact on the development of the social sciences in Japan. Despite these strong Western influences, however, the development of the social sciences and sociology in particular cannot be understood unless we take into account a second influence—the sociopolitical climate in Japan during the twentieth century. This second factor has played an especially important role in influencing the development of a particular subfield of Japanese sociology, *shakai-ishikiron* (the "study of social consciousness"), which does not have a direct counterpart in either European or American sociology. In contrast to many other subfields of sociology which were directly transplanted to Japan from the West, *shakai-ishikiron* is unique in that its development was stimulated by the concern of Japanese sociologists with contemporary social problems and issues rather than by direct Western influence. The purpose of the present paper is to trace the development of *shakai-ishikiron* and analyze it in its relation to the above two forces—Western influence and the contemporary Japanese sociopolitical climate. It is hoped that the present analysis will not only familiarize American readers with this uniquely Japanese subfield of sociology but will also serve as a case study of the more general problem of the development of the social sciences in non-Western nations.

Since many American readers are not familiar with Japanese sociology in general, it will be useful to present a brief overview of the field and discuss current trends before we turn to an examination of *shakai-ishikiron* in particular.

A BRIEF HISTORY OF SOCIOLOGY IN JAPAN AND CURRENT TRENDS IN THE FIELD

The history of Japanese sociology can be divided roughly into two time periods, with the influence of the German school

predominating prior to World War II, and the American school acting as the major foreign influence in the postwar period. In addition to the work of German theorists, the ideas of English and French theorists were also highly regarded in the late nineteenth and early twentieth centuries. The work of T. Takebe (1906) provides one of the best examples of how the early Japanese sociologists adopted and modified influential Western theories according to the philosophical and political foundations of Japanese society. Takebe combined both Eastern and Western conceptions of progress into a general theory of Confucian social evolution, by which each society as an organic unity moves through universal stages of evolution, finally reaching the ideal Confucian state.

Not until the turn of the century did sociology become firmly established as an academic discipline in Japan, with Takebe's establishment of a sociology *koza* (academic division) in 1903 at the University of Tokyo. This was followed by the creation of the Japanese Sociological Academy *(Nihon Shakaigakuin)*[1] in 1913, also by Takebe.

While sociologists at the University of Tokyo continued through the 1920s their initial emphasis on grand organic theories of society, those at Waseda University, Kyoto University, and other schools where sociology *kozo* were newly established became interested in the new trends in both European and American sociology. Of particular interest to many sociologists was the formal sociology of Simmel, Vierkandt, and Von Wiese. Likewise, the development of "psychological sociology" in Europe and the United States, notably by Tarde and Gid-

dings, attracted the attention of many Japanese sociologists.[2]

It was also during the 1920s that an interest in empirical research began to develop, largely through the influence of T. Toda, who had received his training in the United States. Eager to introduce techniques for empirical research in Japan, Toda supervised and conducted many empirical studies, especially in the area of family sociology.

Although empirical research, especially in family and rural sociology, continued to receive emphasis in the thirties, the influence of German sociology was by no means on the decline. Japanese sociology in the thirties reflected the German trend away from formal sociology toward cultural sociology *(Kultursoziology)*, of which the sociology of knowledge was a major component.

Turning to Japanese sociology in the postwar period, it is clear that it has been characterized by the overwhelming influence of American sociology, along with the increasing influence of Marxism. Empirical research methodology and quantitative techniques have been strongly advocated and widely accepted in the postwar years. Also reflecting trends in American sociology, the functionalist orientation gained in popularity during the sixties.

The respective role of domestic and foreign influences in the development of Japanese sociology can best be appraised by a look at the Japanese *Treatises in Sociology (Shakaigaku Koza)*. The 18 volumes of this work can be considered representative of the current state of sociology in Japan. By comparing the number of references each encyclopedic volume, representing a subfield of sociology, makes to works by foreign scholars

---

[1] Membership in the Academy initially numbered around 400, although many members were not professional sociologists. The Academy was replaced in 1923 by the Japanese Sociological Association *(Nihon Shakaigakkai)*, which was more professionally oriented than the Academy. Current membership in the Japanese Sociological Association exceeds 1300, showing an increase of about 50 percent over the past twenty years. When participation at national and regional meetings of the association is used as a criterion, about one-third are active members (Fukutake, 1976).

[2] Spokesmen for the new trends included E. Endo of Waseda, and S. Yoneda and his student Y. Takada at Kyoto University. Although Endo first systematically introduced American sociology and Yoneda contributed the most in terms of the introduction of a variety of Western sociological theories, Takada is the one who is primarily responsible for giving sociology its status as a "common citizen" among the social sciences in Japan.

with the number of references that are made to works by Japanese scholars, we can gain an understanding of the relative "Japaneseness" of each subfield. Although the same article or book may be referred to more than once in the same volume, counting the references nevertheless provides a barometer of the overall attention given to Japanese and foreign works, respectively.

THE DEVELOPMENT OF *SHAKAI-ISHIKIRON*

Shakai-ishikiron literally means the study of social consciousness or social awareness, and as such it encompasses aspects of both social psychology and the sociology of knowledge. *Shakai-ishiki* consists of the mental representations— including attitudes, beliefs, opinions, ideas, values, and knowledge—shared by members of a social group. There is a general consensus among scholars in the field that the word *shakai-ishiki* is comprised of two elements: "social psychology" and ideology, in the sense that these terms are used in Soviet textbooks on historical materialism (Hidaka, 1958; Minami, 1957; Mita, 1971; Shibata, 1961; Sanada, 1965). That is, "social psychology" is comprised of the mental activities shared by members of a social group or society, whereas ideology is the body of mental activities as they are articulated and posited by experts or ideologues. Translated into the language of Western sociology, *shakai-ishikiron* is the study of both the esoteric thought of experts and the popular attitudes and beliefs of a group—the respective subject matters of the European and American variants of the sociology of knowledge as discussed by Merton (1968). In fact, many of the substantive concerns of the sociology of knowledge—dealing with the social derivation of attitudes, opinions, ideas, and knowledge—have accumulated under the label *shakai-ishikiron* in Japan, much as the sociology of knowledge in the United States has declined as a distinct subfield due to the accumulation of related research in other branches of American sociology. It is appropriate, then, to trace the development of *shakai-ishikiron* in Japan starting with the denouement of the sociology of knowledge in the 1930s.

The sociology of knowledge was introduced in Japan in the late twenties and early thirties, immediately following its establishment in Europe and coincident with its introduction in the United States. We may briefly point out two major characteristics of the sociology of knowledge in Japan during this early period: (1) its affiliation with Marxism, and (2) the absence of empirical research. Regarding the latter, it may be pointed out that the active life of the field as distinct from others was so brief, spanning less than two decades, that there was scarcely time to accumulate empirical studies. In addition, empirical orientation in Japanese sociology in general was still in its infancy at this time.

By far the highest percentage of references to Japanese works lies in the area of rural sociology, with 95 percent (346 out of the 366 total) referring to works by Japanese sociologists. This clearly reflects the accumulation of substantive research—both theoretical and empirical—in this area since the thirties.[3]

_____

[3] The second most "Japanese" field is the sociology of law, with 79 percent of the references made to Japanese works. Urban sociology and social planning also rank high in terms of the Japanese contribution (70%), followed by social disorganization (63%), theories in social work (62%), industrial sociology (53%), educational sociology (50%), *shakai-ishikiron* (46%), theories of modern society (42%), economic sociology (37%), theoretical sociology (37%), sociological theory (33%), mathematical sociology (18%), and the sociology of knowledge (14%). Three of the subfields listed above— "theories of modern society," "theoretical sociology," and "sociological theory"—sound deceptively similar when translated into English, but actually there are substantial differences among them. "Theories of modern society" (*gendai shakairon*) as a subdivision of sociology encompasses theories that deal with certain phenomena of contemporary life—mass culture, urban society, and so on. "Theoretical sociology" (*riron shakaigaku*) is the term used for current theoretical trends; structural-functionalism currently occupies a central position in this subfield. "Sociological theory" (*shakaigaku riron*), on the other hand, is very close to what is termed macrosociology in the United States, with studies typically focusing on class relations and other issues in stratification, Japanese modernization, and economic development and industrialization.

The area in which the proportion of references to Japanese works is lowest is the history of sociology; only 9 percent (64 out of 690) are to domestic works. This no doubt reflects the feeling on the part of many Japanese sociologists that their overall contribution to the international development of sociology as a discipline is slight, as well as the fact that the history of sociology in Japan is largely that of successive imports of different trends in Western sociology.

We also may assess the current distribution of interests of Japanese sociologists from the number of articles they publish in each subfield. Although comparison is somewhat restricted by the fact that the subfields in the *Japanese Sociological Review* do not exactly duplicate those used in the *Treatises* mentioned earlier, we can see nevertheless that the volume of publication in a given subfield does not have any apparent relation to its "Japaneseness." Fields with the greatest number of articles published in 1977 were educational sociology (with 119 articles), and the joint field of social philosophy, thought, and history (105).[4] The high volume of Japanese publication in fields such as these and others which are markedly Western in terms of orientation is indicative of the continuing popularity and demand in Japan for Western scholarship and interpretation of Western theories.[5]

Having briefly reviewed the history of

Note that the editors of the *Treatises* separated *shakai-ishikiron* and the sociology of knowledge into two separate volumes. As we will see, *shakai-ishikiron* is a combination of not only American and European variants of the sociology of knowledge, but ideas from American social psychology, German psychoanalytic theory, and Marxism. The low percentage of references to Japanese works on the sociology of knowledge is indicative of the fact that the Japanese contribution has been largely subsumed under the title of *shakai-ishikiron*.

[4] Figures are taken from the "Annual Bibliography of Members' Publications, 1977," the *Japanese Sociological Review*, 29(2), 1978.

[5] The result of a survey conducted by the Japanese Sociological Association of its members in 1973 (cited in Fukutake, 1977) reveals the same pattern of popularity among subdivisions. The six subdivisions of sociology which ranked highest in terms of the number of members who considered themselves specialists were: (1) rural and community

sociology in Japan and the current emphases in the field, we turn now to an examination of the uniquely Japanese subfield *shakai-ishikiron*.

Those who played a major role in the introduction of the sociology of knowledge in Japan were young sociologists who formed an academic group called *Shakaigaku Kenkyukai* (the Society for Sociological Research). The majority of the scholars in this group were men who had started their college education in the early 1920s and had begun their professional academic lives around the end of the decade. Thus, most of them were young academics during the rise and fall of Taisho democracy, the liberal political period immediately following World War I. The 1920s in Japan were characterized first by liberalism and then by successive economic depressions. Facing increasingly difficult social problems and issues, social scientists began to focus more on the concrete problems of society such as welfare and poverty rather than on the pure abstract theories which had previously occupied their attention. Moreover, Marxism, which had been introduced to Japan around the turn of the century, began to increase in influence among social scientists and student radicals. The strength of its appeal among social scientists and especially among sociologists was partly reinforced by the inability and inattention of traditional Japanese sociologists to face complex social problems in their work; younger sociologists saw in the Marxist approach a relevance to contemporary social issues which they found lacking in Japanese sociology at the time. Due to strong opposition from the government, however, the influence of Marxism subsided toward the end of the decade, at least on the surface. One result was that the sociology of knowledge became particularly appealing to those sociologists who were sympathetic to Marxism, but who no longer felt free openly to pursue Marxism in academic

sociology, (2) sociology of the family, (3) social philosophy, thought, and history, (4) social problems, (5) industrial sociology, and (6) social psychology and *shakai-ishikiron*.

circles. Although the sociology of knowledge had originally emerged in Europe as an antithesis to the Marxian theory of ideology, this aspect of its origins was obscured in its importation to Japan. Akimoto (1973) even argues that its close affiliation with Marxism left the sociology of knowledge on the level of ideological debunking in Japan and made it difficult for it to gain full-blown status as an academic subfield within Japanese sociology. Whether this assessment is accurate or not, the sociology of knowledge as an active field declined prior to World War II. Many aspects of it were retained, however, in the new *shakai-ishikiron* which emerged in the immediate postwar years.

As we will see, *shakai-ishikiron* as the postwar offspring of the sociology of knowledge also became heavily imbued early on with a Marxian orientation. The liberalization of the sociopolitical environment in the postwar years encouraged and sustained the new field of *shakai-ishikiron*, whose proponents directed much of their research effort into critical explorations of prewar Japanese social thought and ideology.

Before going on to examine the development of *shakai-ishikiron*, it will be instructive to mention briefly the role of Marxism in the Japanese academic world, including sociology. One can offer many reasons to explain the significant role Marxism has played in Japan, but it may be convenient to divide them into three types: (1) cultural reasons, (2) intellectual reasons, and (3) social-structural reasons.

Regarding the growth of Marxism during the prewar period, Fairbank et al. (1964) have pointed out that: "One reason is that there was a certain resonance between Marxism and orthodox Japanese thought. In both the individual is slighted, subordinated to his social group. In Marxist groups in Japan the demand to sacrifice individual goals for the 'good of the people' was almost as compelling as the demand in other contexts to sacrifice 'for the state'." They go on to mention, "The weakness of individualism in modern Japan, together with the similarity between the traditional collectivity, with the unspoken identification of the individual and group, and the ideal communism of

Marx . . ." (Fairbank et al., 1965:552–553). Certain consistencies between Japanese culture and the social implications of Marxism, or at least the perception of such consistencies, then may be one reason why Marxism gained a foothold in the early part of the twentieth century in Japan.

It is also important to note the way in which the political heritage of Japan has influenced the particular role played by Marxism and the characteristics of its adherents. The Meiji Restoration essentially laid the modern political foundations of Japanese society in the late nineteenth century. In contrast to the modern revolutions of France and the United States, fought in the name of liberal political ideals such as liberty and equality, the Meiji Restoration was led by those elements of the society whose primary goal was to strengthen the nation vis-à-vis the Western threat. Internal reorganization of the political structure was also a central goal, but was framed in terms of national unity rather than individual freedoms. Broadly speaking, because the government has continued to be largely represented by conservatives ever since the establishment of national unity, and liberals have frequently found themselves outside the system, there has been a structural affinity between liberals and more radical, Marxist-oriented elements. As a result, there has been a recurring tendency for liberals and Marxists to join forces, both within and without the academic world, in Japan. The founders of *shakai-ishikiron* were among those liberals who favored an intellectual coalition with Marxist-oriented scholars in the late 1940s and the 1950s.[6]

In terms of intellectual traditions, as we mentioned earlier, evolutionary paradigms of the state had aroused considerable interest in Japanese academic circles in the early twentieth century, and the historical orientation in Japanese

---

[6] This cooperation between liberal and Marxist elements is, of course, a phenomenon which is not unique to Japan, but can be observed in many "developing" nations in which unification was achieved through conservative forces, and in which a liberal tradition did not provide the initial basis for national identity.

sociology was strong from the start. The introduction of Marxian thought was quite natural, given this background, and also given the fact that, again in Fairbank, Reischauer, and Craig's terms, "science was highly esteemed by all, and German philosophy was widespread" (1965:553).

Finally, certain characteristics of Japanese social structure undoubtedly play some role in the continuing influence of Marxian thought in Japanese academia. One factor which may be conducive to Marxian thought in academic circles is the distance between the Japanese governmental bureaucracy and the academic world. Typically one is either a bureaucrat or a scholar in Japan but not both; individuals who fill both positions during their lives are few in number. This discrete separation between state bureaucracy and academics would seem to be a context in which Marxian theory, with its criticism of the state, would flourish.

Given these comments, we are ready to consider the postwar development of *shakai-ishikiron* and its relationship to the political and intellectual climate in Japan.

The Foundations of Shakai-Ishikiron: Marxism and Social Psychology

Just as the emergence of the sociology of knowledge in Germany and its gradual acceptance in the United States have been analyzed in terms of the contemporary social and political climate in the two countries, so can we trace the development of *shakai-ishikiron* in Japan in the years immediately following the Second World War. Mannheim (1946) saw the sociology of knowledge as an outgrowth of the 1920s in Germany, where antagonistic intellectual factions were trying not only to refute each others' ideas, but the very underpinnings and intellectual foundations of such ideas. Likewise, Merton (1968) pointed out that the increasing social conflicts and differences in attitudes and values among socially differentiated groups in American society in the thirties and forties provided a social context ripe for the introduction of the sociology of knowledge to American sociologists. In a similar fashion, we can see the social context for the formation of *shakai-*

ishikiron in the tremendous changes and conflicts in values, attitudes, and ideals which came about in the immediate postwar period in Japan. Rapid and dramatic reordering of social and economic institutions occurred as a result of the reforms effected by the Occupation forces and the consequent disruption of the prewar establishment. In the intellectual, social, and political confusion, the relativity of social and political values became increasingly clear to many. Under these circumstances, the criticism of prewar ideology by Marxist scholars gained wide acceptance among sociologists as well as within the non-academic community. Criticism of the prewar ideology of Emperor worship became widespread, and Marxist scholars articulated the view that the *zaibatsu* or big business interests in Japan had supported the ideology behind the war because of the huge profits they were able to make from wartime enterprises. In short, Marxist scholars were outspoken in their attempt to show the public that what had been accepted and adopted by the general public as absolute truth in the prewar and wartime periods had been none other than a reflection of the publicity strategies of various interest groups in Japanese society.

More important than the baring of ideology that occurred, though, was the fact that people became more aware of the possibility that what they believed to be their own inner convictions and voluntary actions were influenced profoundly by forces outside of themselves, forces beyond their own control. Although the critics of prewar ideology did answer— whether correctly or not—the question of what had produced the particular ideology and what interests had been served by it, they could not answer the question of *why* ordinary people had accepted and supported the ideology. The development of *shakai-ishikiron* was stimulated by such questions as how was the prewar ideology produced and why had its proponents been able to gain such mass acceptance for it. *Shakai-ishikiron* was a natural product of Japanese sociologists' attempts to answer these questions, especially those sociologists who were not fully satisfied with the analysis provided by

Marxist critics, and who sought to establish why the public had been so accepting of ideologies which supported the war.[7]

This question of why and through what processes an ideology comes to be accepted by the common public is one which had been vigorously pursued by the Frankfurt School and by many of the European neo-Freudians. The founders of *shakai-ishikiron* were naturally attracted, then, to works by such scholars as Adorno and Fromm, and early writings in the field bear the strong influence of the work of members of the Frankfurt School on the authoritarian personality.

The Japanese scholars who played the major role in the founding of *shakai-ishikiron* in the late forties and early fifties were I. Shimizu, R. Hidaka, H. Minami, and A. Takahashi. With the exception of Shimizu, who had been active as a sociologist and journalist since the early thirties, these were all men whose academic careers had started during the forties and who were thus young scholars. Besides being responsible for the early development of *shakai-ishikiron*, they also introduced social psychology into Japan. Thus social psychology and *shakai-ishikiron* were not clearly separated in the works of these scholars, particularly in the fifties; some of them even held the belief that *shakai-ishikiron* would become a unique Japanese brand of social psychology.[8] A striking characteristic of the founders of *shakai-ishikiron* was their optimism concerning the integration of Marxism and American social psychology. One scholar criticized the apparent aspirations of these men in the following words: "In the theoretical work of mainly the Tokyo University sociologists and also in the thinking of Minami and his followers, there seems to be the rather unrealistically optimistic idea that American social science and Marxian theory can be welded into a uniquely Japanese tool which can implement a social revolution reorganizing Japanese society" (Wagatsuma, 1969:42).

Shimizu was the leader in attempting a synthesis of the Marxian approach and American social psychological concepts. After publishing *Shakaigaku Hihan* (Criticisms of Sociology) in 1933, written from a Marxian viewpoint, he studied American social psychology intensively during the war years. In his postwar works he attempted to combine American social psychological theories and concepts he had studied during the war within his original Marxian framework. The course he set was followed and expanded upon by Minami, Hidaka, Takahashi, and their students.

Minami, who had studied psychology at Cornell and returned to Japan in the late forties, was among the first to systematically introduce American social psychology to the Japanese public. His principal concern in the immediate postwar years was how to transform the traditional "feudal" mentality of the Japanese people into a modern, Western, "democratic" mentality. For Minami, the traditional character of the Japanese people constituted one of the greatest obstacles to the construction of a modern democratic nation. This concern was not unique to Minami, but was one which was rather commonly shared by the so-called progressive intellectuals of whom Shimizu was the leader.

Combining psychoanalytic personality theory with a Marxian perspective on the formation of ideology, Minami pointed out that the acceptance of a particular ideology in a society is facilitated by the existence of certain personality types, which in turn are the result of the specific economic

---

[7] Although political scientists such as M. Maruyama and his followers were interested in these and similar questions (c.f., Maruyama, 1956), discussion in the present paper will be limited to the sociological field.

[8] These sociologists at the University of Tokyo, as well as Minami and his followers, considered themselves social psychologists. For this reason, the study of *shakai-ishiki* or the attitudes, opinions, beliefs, and values held by groups of people became almost synonymous with social psychology in general in Japanese sociology. Although the experimental tradition in social psychology is also strong, scholars conducting experimental research generally belong to the Japanese Psychological Association, while those interested in the study of *shakai-ishiki* belong to the Japanese Sociological Association and the Japanese Social Psychological Association. In addition, researchers in small group research belong to the Society for the Study of Group Dynamics. Social psychology and *shakai-ishikiron* are still considered by the Japanese Sociological Association to make up together a single subdivision of sociology.

and social conditions under which people are socialized (Minami, 1957). He thus attributed particular weight to the role of historical conditions in the formation and acceptance of ideas. At the same time, in Minami's scheme, ideology is an integrated system of attitudes and can therefore be analyzed with the same techniques used in the psychological study of attitudes. He thus attempted to explain the growth and acceptance of an ideology or *shakai-ishiki* in terms of social psychological mechanisms of attitude formation, while emphasizing that the psychological variables utilized are always relative to the specific social and economic conditions prevailing in society at the time.

It was through the work of Hidaka and his colleagues and students at the University of Tokyo—Takahashi, J. Watanuki, K. Kido, and M. Sugi—that the paradigm of *shakai-ishikiron* became firmly established in the fifties. For this group of scholars, the relationships among ideology, personality, and social structure became the central problem of the sociological endeavor. Accepting the basic Marxian framework in regard to the relationship between the infrastructure and the superstructure, they assigned to personality a role juxtaposed between these two. According to Hidaka (1958), although the Marxian approach and the social psychological approach are basically complementary, there is one fundamental discrepancy between the two. Whereas the Marxian approach considers ideological consistency to be consciously and rationally sought by its adherents, the social psychological conception of ideology is that it is an empirical but not necessarily conscious consistency among attitudes and opinions. Hidaka argues that Marxists, with their emphasis on the logical and conscious aspects of ideology, tend to ignore the unorganized attitudes and opinions of ordinary people, whereas social psychologists tend to get lost in a flood of diverse and unorganized attitudes held by the public. For Hidaka, the contribution of Marxism is its emphasis on the material determination of ideology, and the contribution of social psychology is its recognition of the importance of personality in sustaining ideology. In his view,

only when the two approaches are somehow integrated in a theoretical framework can *shakai-ishiki* be meaningfully analyzed.

Empirical Research in the Field of Shakai-Ishikiron

The founders of *shakai-ishikiron* were strong advocates of empirical research, both qualitative and quantitative. Minami and his followers have tended to produce mostly qualitative works, based on participant observation and content analysis, while Hidaka's group has been more quantitatively oriented. The latter group was especially active in importing psychometric and attitude scaling techniques that had been developed in the United States. Since its inception as a field, the bulk of the empirical research in *shakai-ishikiron* has been concentrated on the study of attitudes and opinions, the exception being the content analysis studies of newspapers, magazines, popular books, and popular songs (e.g., Minami, 1963, 1969; Ikeuchi et al., 1956, 1957; Mita, 1965; Sakuta, 1972; Yamagishi, 1979). We briefly mention here a few studies in order to illustrate the types of topics undertaken in empirical research in the field.

Early empirical studies in *shakai-ishikiron* (in the fifties and early sixties) focused particularly on workers' and farmers' attitudes and were characterized by a strong Marxian flavor. Hidaka and his group were the first to employ systematic techniques of attitude measurement; in their attitude study entitled "Changes and Continuities in Traditional Mentality," they attempted to show that a traditional type of mentality had been activated among workers by "political reactionaries" to the socialist movement of the late forties and fifties (Hidaka et al., 1954).

Psychometric and other new techniques were adopted by Hidaka's group, Kido being the leader in this respect.[9] In their

---

[9] The Kido Award, in honor of K. Kido who died in his early thirties, became the most prestigious award in Japanese social psychology. This award, however, was terminated in 1979. Considering the fact that the award has been given mostly to works in

study of the attitudes of steel workers, Kido and Sugi (1945) utilized the Japanese Authoritarianism Scale, and the Political and Economic Ideology Scale, two relatively well-constructed measurement devices. In their study, they pointed out that the relationship between these two scales depended on other sociological variables, such that the correlation between the two was relatively high for certain groups of people but low for others; the specific finding of their study was that politically radical workers were not necessarily less authoritarian than conservative ones.

Another group of sociologists who vigorously pursued empirical research under the label of *shakai-ishikiron* is comprised of several specialists in rural sociology, notably T. Fukutake, T. Hosoya, and M. Shimazaki. According to their interpretation of Marxian theory, the "material determination of ideas" means that the particular problems which people confront and likewise the solutions which they devise are intimately related to their material interests and position in society. So, in *shakai-ishikiron* or the analysis of social consciousness, it is not enough to simply compare the distribution of attitudes and opinions among different social groups. Instead, there must be an attempt to link the particular distribution of attitudes to the problems and conflicts specific to people in different groups and situations. They further argue that analyses of *shakai-ishiki* should not be undertaken until one gains an understanding of the nature of the problems faced by the people in the social and historical context one is studying (c.f., Shimazaki, 1960, 1961; Hosoya, 1962; Hamajima, 1960). Consistent with this orientation, the central focus of the research undertaken by these sociologists has been the attitudes and opinions of Japanese farmers facing the process of polarization within the agricultural community—the stratification of agriculture into small vs. large enterprises. (See Fukutake, 1977, for a list of empirical studies in this field.)

Recent Trends in Shakai-Ishikiron

During the 1960s, there was an important shift in the substantive concerns of *shakai-ishikiron*, mirroring changes in Japanese society itself. Rapid economic growth and population movement from rural to urban areas was accompanied by the growing influence of the mass communications media, especially television. As a result, mass communications and mass society became popular topics in sociology. Students of *shakai-ishikiron* were influenced by this new trend, and interest in analyzing traditional or "feudal" mentality declined in light of the new concern with "alienation" in mass society. The attraction was mutual: the mass media welcomed the interest demonstrated by sociologists as an indication of the important role played by mass communications in modern Japanese society. *Shakai-ishikiron* achieved journalistic popularity as students of the field assimilated the works of Western scholars such as David Riesman, Seymour Martin Lipset, C. Wright Mills, and William Alan Kornhauser.

Another reason for the popularization of *shakai-ishikiron* in the sixties was its focus on the study of Japanese national character, a topic which has retained its popularity in both academic and non-academic circles since the late nineteenth century. (See Hamaguchi, 1965, for post-war studies of Japanese national character.) Prior to the fifties, most of the studies of national character were of rather an impressionistic nature and tended to be methodologically weak. Systematic techniques of observation were introduced during the fifties by cultural anthropologists, and this was followed by the widespread adoption of quantitative techniques in the sixties. Much of the methodology (such as psychometric techniques) for studying national character was borrowed from *shakai-ishikiron*, such that the quantitative study of Japanese culture and mentality came to be generally considered the domain of *shakai-ishikiron*.

We feel it safe to say that the empirical sophistication of the quantitative research on Japanese national character has not

been adequately matched by strength in the theoretical realm.[10] Although research techniques were rapidly improved during the sixties, when the national government and large newspapers and television networks provided extensive funds for research into national character, the tendency was to employ relatively advanced analytical techniques largely for the description of attitudes and opinions.[11] The implications of empirical studies for theoretical works was not given high priority. Despite the resulting vacuum in the theoretical area, or perhaps because of it, research money continued to go into empirical studies, and they came to constitute the mainstream of current research in *shakai-ishikiron*. The close tie between theory and empirical research advocated by the founders of *shakai-ishikiron* was thus gradually replaced by a concentration on the part of many contemporary researchers on the refinement of methodology, with less emphasis placed on theoretical issues. The result was what could be termed the dichotomization of theory and empirical research in the sixties, and this split has continued to characterize the state of *shakai-ishikiron* throughout the seventies.

The fissure between theoretical and empirical work in the field of *shakai-ishikiron* in fact has grown deeper during the seventies. On the one hand, the theoretical research has become more social philosophical in nature, while on the other hand, empirical research has remained largely descriptive rather than oriented toward the testing of theories and hypotheses.

Representative of current theoretical work, for instance, is Mita's "*Shakai-Ishiki* in Modern Society," in *Treatises in Sociology*. This essay is an abstract discussion of modern society and "modern social consciousness." As such, it presents a particular outlook on contempo-

rary society, but does not go beyond this to provide analytical concepts or a theoretical framework that can stimulate further research. According to Mita, in studying modern society we should first look at the conflict within "modern individuals." These individuals constitute the "civil society," which can be analyzed best in terms of the market relations among the producers and owners of capital goods and private property. The *shakai-ishiki* of these "modern individuals" thus should be seen as the consciousness specific to the various classes in a capitalist system. Finally, we are advised that changes in *shakai-ishiki* should be analyzed from the viewpoint of the dynamics of "control" exercised by the higher class and the "realization of totality" on the part of their subordinates in society. In conclusion, "*shakai-ishiki* in modern society can be seen as a battlefield between two types of rationalization, two ways of comprehending the world in its totality; i.e., a battlefield between 'control' and the 'realization of totality' " (Mita, 1976:23).

One of the factors responsible for the split between the theoretical and empirical factions in *shakai-ishikiron* may be the general attitude of "progressive intellectuals" who "sometimes tend to program for others what they themselves cannot actually do" (Wagatsuma, 1969:42). In other words, there may be a tendency on the part of intellectuals in Japan and elsewhere to draw up such abstract plans for social reform that no one either within or without the academic community becomes committed to trying to implement them. Unfortunately this can result in alienation between practically and theoretically oriented groups who, ideally, should serve complementary functions. A second factor which may contribute to the dichotomization between theory and empirical research in the field of *shakai-ishikiron* is the weak training in methodology and its applications in many Japanese sociology departments. As the result of an abbreviated background in research methods and statistical reasoning, many theorists in *shakai-ishikiron* tend to equate statistical analyses with numerical *descriptions*. It is understandable, then, that this group finds the statistical studies

---

[10] A series of studies conducted by the Tokei Suri Kenkyujo (Institute for Mathematical Statistics) serves as a good example (c.f., Tokei Suri Kenkyujo, 1961, 1970).

[11] Although dimensional analyses such as factor analysis, cluster analysis and multi-dimensional analysis are popular, they are used as techniques for the description of data, not for hypothesis testing.

of the empirical faction unacceptable and lacking in usefulness.[12]

The critical stance on the part of the theoretical faction concerning research methodology and statistical reasoning also must be due in part to the actual way in which members of the empirical faction use empirical techniques. As we have said, although the techniques employed are quite sophisticated, they generally are used for descriptive purposes. That is, the strategy of testing hypotheses derived from a theory and then modifying the theory on the basis of empirical research findings is not the usual pattern that is followed in their research; perhaps, one could argue, because there is a dearth of theories from which to generate hypotheses. There is thus a disappointing lack of cross-fertilization of ideas between the two factions in *shakai-ishikiron*.

SHAKAI-ISHIKIRON IN COMPARATIVE PERSPECTIVE

Having discussed the influence of diverse forces on the development of *shakai-ishikiron* in the postwar period, we will close by exploring briefly the differences between this field and its closest contemporary counterpart in both American and European sociology—the sociology of knowledge. Through this comparison, the characteristics of *shakai-ishikiron* as a Japanese hybrid will become clear. In particular, it will become evident that the two factions of the Japanese school, the theoretical and the empirical, are in some sense microcosms of the two foreign schools. We will look first at the general approach taken toward research by scholars in each of the foreign schools and then deal with the philosophical and political traditions represented by the schools and their general sense of purpose.

The points discussed by Merton (1968) in his analysis of the differences between the orientation of American and European sociologists of knowledge will be useful as a framework. Merton examines four areas: (1) the subject matter and definition

of problems, (2) the attitudes regarding data and facts, (3) research techniques and procedures, and (4) the social organization of research activities.

First, according to Merton, American specialists in the sociology of knowledge focus especially on the consumption and diffusion of popular beliefs and ideas, while European sociologists are more interested in the esoteric doctrines of the intellectual elite. On this dimension, *shakai-ishikiron* as a whole is closer to the American variant; we have seen that its principal subject matter consists of the opinions and attitudes of ordinary people. Second, in regard to the approach taken toward data, American sociologists are concerned with using reliable empirical data, "even at the price of surrendering the problem which first led to the inquiry," while the European tendency is to rely heavily on impressionistic data, if necessary, in the pursuit of macro-level problems (Merton, 1968:445). In this respect, the Japanese *shakai-ishikiron* represents elements of both the American and European traditions, as we have seen, but is divided into factions on this particular dimension. Third, the problem of reliability of data, which is of central concern to American researchers, is given less weight in the European variant of the sociology of knowledge. On this point, as we also have seen, the division of *shakai-ishikiron* into factions is clear. Finally, concerning the social organization of research, the lone European scholar "is not constrained *by the very structure of his work situation* to deal systematically with reliability as a technical problem," while the organized research team in the United States "forces attention to such technical problems as reliability" (Merton, 1968:453). This distinction made by Merton corresponds, again, to the division between the theoretical and empirical factions in *shakai-ishikiron*.

In regard to the philosophical and political orientation of sociologists of knowledge, Kurt Wolff expresses the view that two tendencies "have characterized most, though not all, American writings in the field: *impatience with epistemology* and rejection of the idea that the sociology of knowledge occupy itself

---

. [12] See Yasuda (1970) concerning the kinds of misconceptions regarding statistical reasoning that are held by the theorists of *shakai-ishikiron*.

with it, and perhaps more than animosity, *indifference toward Marxism and ignorance of it*" (Wolff, 1967:7). In addition, Wolff also points out the prevalence of "an ahistorical-systematic, rather than a historical approach" in the American school. These tendencies are in implicit contrast with the sociology of knowledge as it is practiced in Europe.

As for the "impatience with epistemology," *Shakai-ishikiron* is quite similar to the American variant of the sociology of knowledge. Even scholars in the theoretical faction give little consideration to epistemological questions, especially those specifically concerned with the social derivation of knowledge. The lack of attention to questions of epistemology in both the United States and Japan, interestingly enough, stems from very different causes. In the United States, it often is argued that the sociology of knowledge, as the scientific study of thought, should deal only with the relationship between mind and environment, and that the validity of an idea or ideology is irrelevant in the study of this relationship (c.f., Adler, 1957; Becker and Dahlke, 1942; Child, 1947; DeGré, 1941; Hinshaw, 1948). In Japan, on the other hand, the absence of interest in questions of epistemology is a result of the almost total commitment to the Marxian framework. It is assumed implicitly in the writings of the theoretical faction that the truth of knowledge should be judged in reference to Marxian theory and the Marxian analysis of the socioeconomic structure of society. The final criterion of the validity of an idea lies in its correspondence to material conditions. In its most radical formulation, the role of *shakai-ishikiron* is to help people realize the existing discrepancy between objective reality—which can be correctly analyzed only from the Marxian viewpoint—and their perception of it (c.f., Hosoya, 1962; Tanaka, 1965).

Finally, as we have seen, *shakai-ishikiron* has a historical, macro orientation, and thus it is much more similar to the European sociology of knowledge in this respect than to the American. For the scholars in the theoretical faction of *shakai-ishikiron*, empirical research with-

out reference to historical factors would be devoid of significance.

Psychological reductionism is another characteristic of the American school, as viewed by some observers (Chall, 1958; Curtis and Petras, 1972). Chall (1958) argues that, because of its systematic negligence of social as distinct from psychological mechanisms, the American school represents only in part the sociology of knowledge, and that the sociology of knowledge has in large part become a latent frame of reference for the science of man. Although psychological variables are widely utilized both in the theoretical writings and empirical research in *shakai-ishikiron*, a characteristic of *shakai-ishikiron* in this respect is the strong influence of Freudian psychology in contrast to that of Meadian psychology. Although symbolic interactionism has some influence among Japanese social psychologists and students of communication, the paradigm seems to be dissonant with the basic theoretical framework of *shakai-ishikiron*.[13] First of all, whereas micro-level phenomena provide the material for the symbolic interactionist approach, *shakai-ishikiron* has focused primarily on macro-level questions. Second, the introduction of symbolic interactionism in Japan took place relatively recently (in the 1960s), well after the basic paradigm of *shakai-ishikiron* already had been established. Although symbolic interactionism recently has attracted some attention from Marxists, it did not have a strong influence on those Marxist-oriented sociologists who created *shakai-ishikiron* as a sociological subfield in the immediate postwar period.

Although the sociology of knowledge as such has been disappearing from sociology in Japan as in the United States, related studies—both theoretical and empirical—dealing with problems to which the sociology of knowledge origi-

---

[13] Even Miyajima, who sees the possibility for the future development of *shakai-ishikiron* in its affiliation with phenomenological sociology, states that "it was only when the American empirical-individualistic study of *ishiki* was coupled with Freudian psychology that the social-psychological approach to *shakai-ishiki* was really established for the first time" (Miyajima, 1976:182).

nally addressed itself are still generally considered to constitute a distinct subdivision of sociology, i.e., *shakai-ishikiron*. This is one of the major differences in the development of the sociology of knowledge in the United States and in Japan. That is, it has lost its center of gravity and has been dispersed into various subfields of sociology in the United States, while it still exists as a relatively coherent body of research in Japan even though the label has changed.

In closing, we have examined briefly the history of sociology in Japan and, in particular, the development of the unique Japanese subfield *shakai-ishikiron*. *Shakai-ishikiron* can be seen as representative of Japanese sociology in general in terms of its susceptibility, or perhaps its openness, to changes in Japanese society itself—such as changes in the popular perception of prewar ideology and the rapid diffusion of mass consumer culture. Also, it is significant that this blend of the traditions of the sociology of knowledge, neo-Freudian psychology, American social psychology, and Marxism has survived and continues as a recognized specialization in Japanese sociology. By contrast, the sociology of knowledge as its closest American relative has lost its center of gravity, as other subfields of sociology in the United States have gradually come to occupy more and more territory. Whether or not *shakai-ishikiron* will continue to mature as a distinct subfield will depend in part on whether the ongoing process of dichotomization between the theoretical and empirical factions can be arrested and a complementarity between theory and methodology can be achieved.

REFERENCES

Japanese Publications
Akimoto, Ritsuo
    1973   "Mannheim." Pp. 361–393 in R. Akimoto and S. Tanaka (eds.), Chishiki Shakaigaku (The Sociology of Knowledge). Tokyo: Aoki Shoten.
Fukutake, Tadashi (ed.)
    1977   Sengo Nihon no Nōson Chōsa (Rural Research in Postwar Japan). Tokyo: Tokyo Daigaku Shuppankai.
Fukutake, Tadashi
    1976   "Nihon Shakaigaku" (Japanese Sociology). Pp. 253–274 in T. Fukutake (ed.), Koza

Shakaigaku 18: Rekishi to Kadai (Treatise in Sociology 18: History and Problems). Tokyo: Tokyo Daigaku Shuppankai.
Fukutake, Tadashi and Rokurō Hidaka
    1952   Shakaigaku: Shakai to Bunka no Kiso Riron (Sociology: Elementary Theories of Society and Culture). Tokyo: Kōbunsha.
Hamajima, Akira
    1960   "Sengo nihon no kaikyū-kaisō kankei to sono dōtai: Shakai-ishiki no kitei ni aru mono" (Class-stratification relations in postwar Japan and their changes: Foundations of *shakai-ishiki*). Pp. 9–55 in T. Fukutake (ed.), Nihonjin no Shakai-Ishiki (*Shakai-Ishiki* of the Japanese People). Kyoto: Sanichi Shobō.
Hidaka, Rokurō
    1951   "Ideorogī to pāsonaritī" (Ideology and personality). Shisō 319:79–88.
    1956   "Kotonatta gakumonteki tachiba no kyōryoku: Shakaigaku no baai" (Cooperation between different academic points of view: The case of sociology). Shisō 380:1–15; 382:14–27.
    1958   "Ideorogī, shakai shinri, shakaiteki seikaku" (Ideology, social psychology, and social character). Shisō 403:57–68.
    1960   Gendai Ideorogī (Modern Ideology). Tokyo: Keisō Shobō.
Hidaka, Rokurō, Junnosuke Masumi, and Akira Takahashi
    1954   "Kyūishiki no henyō to onzon" (Changes and continuities in traditional mentality). Nihon Shihonshugi Koza 9:163–268.
Hidaka, Rokurō, Kōtarō Kido, Akira Takahashi, and Jōji Watanuki
    1955   "Rodō kumiai to seiji ishiki" (Political awareness of union members). Shisō 373:37–56.
Hosoya, Takashi
    1962   "Nōmin ishiki no henyō to teitai" (Changes and stagnation in farmers' mentality). Shisō 457:48–69.
Ichikawa, Kōichi
    1979   "Nihonjinron no shakai shinrishi no kokoromi" (A social-psychological history of the study of Japanese national character). Hitotsubashi Ronsō 81(2):61–78.
Ikeda, Yoshisuke
    1966   "Sengo Nihon shakaigaku no sōkatsu to tenbō" (Summary and comment on postwar Japanese sociology). Japanese Sociological Review 17(2):26–33.
Ikeuchi, Hajime, Kaiko Okazaki, and Shoko Kubo
    1956,7 "Senryō kikan ni okeru Nihon shinbun no sūkō" (Newspaper trends in Japan during the period of occupation by the Allied Forces). Tokyo Daigaku Shinbun Kenkyūjo Kiyō 5 & 6:83–116.
Kawamura, Nozomu
    1966   "Sengo Nihon shakaigaku to marukusushugi" (Postwar Japanese sociology and Marxism). Japanese Sociological Review 17(2):34–43.
Kido, Kōtarō
    1970   Shakai-Ishiki no Kōzō (The Structure of Shakai-Ishiki). Tokyo: Shinyōsha.

Kido, Kōtarō, and Masataka Sugi
1954 "Shakai-ishiki no kōzō (The structure of shakai-ishiki). Japanese Sociological Review 13 & 14:74–100.
Kurauchi, Kazuta
1943 Bunka Shakaigaku (Cultural Sociology). Tokyo: Baifūkan.
Maruyama, Masao
1956 Gendai Seiji no Shisō to Kōdō (Thought and Practice in Modern Politics). Tokyo: Miraisha.
Minami, Hiroshi
1949 Shakai Shinrigaku: Shakai Kōdō no Kiso Riron (Social Psychology: Basic Theories of Social Behavior). Tokyo: Kōbunsha.
1952 "Tennōsei no shinriteki kiban" (Psychological foundations of the emperor system). Shisō 336:554–563.
1957 Taikei Shakai Shinrigaku (The System of Social Psychology). Tokyo: Kōbunsha.
1963 Shakai Shinrigaku no Seikaku to Kadai (Nature and Problems of Social Psychology). Tokyo: Keisō Shobō.
1964 "Shinrigaku ni okeru higashi to nishi" (The East and the West in psychology). Shisō 478:447–458.
1969 "Ryūkōka kara mita Nihonjin no shinri" (Popular songs and psychology of the Japanese people). Gendai no Esupuri 38:112–134.
Mita, Munesuke
1965 Gendai Nihon no Seishin Kōzō (Mentality of Modern Japan). Tokyo: Kōbundō Shinsha.
1968 "Shakai-ishikiron." In Jōji Watanuki and Jirō Matsubara (eds.), Shakaigaku Kenkyū Nyūmon (Introduction to Sociological Research). Tokyo: Tokyo Daigaku Shuppankai.
1971 Gendai Nihon no Shinjō to Ronri (Feelings and Logic in Modern Japan). Tokyo: Chikuma Shobō.
1976 "Gendai shakai no shakai-ishiki" (Shakai-ishiki in modern society). Pp. 1–26 in M. Mita (ed.), Shakai-Ishikiron. Tokyo: Tokyo Daigaku Shuppankai.
Miyajima, Takashi
1976 "Shakai-ishiki kenkyū no hatten to genjō" (The development and current state of the study of shakai-ishiki). Pp. 162–202 in M. Mita (ed.), Shakai-Ishikiron. Tokyo: Tokyo Daigaku Suppankai.
Nakane, Chie
1958 "Minashigo Nihon no ninshiki" (Japan as an orphan in the world). Chūō Kōron (October):32–45.
Ozeki, Sanpei
1966 "Sengo shakaigakkai ni okeru marukusushugisha no teichaku to teitai" (Acceptance and slow development of Marxism in postwar Japanese sociology). Japanese Sociological Review 17(2):20–25.
Sakuta, Keiichi
1967 Haji no Bunka Saikō (Re-Examination of the Culture of Shame). Tokyo: Chikuma Shobō.

1972 Kachi no Shakaigaku (Sociology of Values). Tokyo: Iwanami Shoten.
Sanada, Naoshi
1965 "Shakai-ishiki to shakaiteki sonzai" (Shakai-ishiki and social existence). Pp. 11–69 in S. Tanaka (ed.), Shakai-Ishikiron. Tokyo: Aoki Shoten.
Sasaki, Ayao
1964 "Chishiki shakaigaku no hōhō ni tsuite no ishikōsatsu (On the methodology of the sociology of knowledge). Japanese Sociological Review 15(2):9–31.
Shibata, Shingo
1958 "Wagakuni ni okeru ideorogīron kenkyū no rekishito kadai" (History and problems in the study of ideology in Japan). Shisō 403:189–203.
1961 Nigensei to Jinkaku no Riron (A Theory of Human Nature and Personality). Tokyo: Aoki Shoten.
Shimazaki, Minoru
1960 "Nōmin no ishiki" (Attitudes and opinions of farmers). Pp. 169–199 in Tadashi Fukutake (ed.), Nihonjin no Shakai-Ishiki (Shakai-Ishiki of the Japanese People). Kyoto: Sanichi Shobō.
1961 "Ishiki kenkyū ni tsuite no kansō" (Comments on the study of ishiki). Japanese Sociological Review 43:129–137.
Shimizu, Ikutaro
1933 Shakaigaku Hihan Josetsu (Introduction to the Criticism of Sociology). Tokyo: Risōsha.
1950 Shakaigaku Kōgi (Lectures on Sociology). Tokyo: Iwanami Shoten.
1951 Shakai Shinrigaku (Social Psychology). Tokyo: Iwanami Shoten.
1958 "Gendai shakai to ideorogī" (Modern society and ideology). Shisō 403:2–20.
Suzuki, Hiroshi
1966 "Sengo Nihon shakaigaku no mondai jōkyō" (Problems and issues of sociology in postwar Japan). Japanese Sociological Review 17(2):2–19.
Takashi, Akira
1956 "Ideorogī kenkyū ni kansuru nōto" (A note on the study of ideology). Pp. 155–177 in Keikai Hayashi (ed.), Nihon Shakaigaku no Kadai (Problems in Japanese Sociology). Tokyo: Yūhikaku.
Takahashi, Akira, and Ryūichi Kitagawa
1957 "Shakaigaku kenkyū no dōkō" (Trends in sociological research). Shisō 398:1222–1229.
Takebe, Tongo
1906 Riron Hutsu Shakaigaku I: Shakai Rigaku (General Theoretical Sociology I: Social Physics). Tokyo: Kinkodo.
Takeda, Ryōzo
1948 Chishiki Shakaigaku no Tenkai (The Development of the Sociology of Knowledge). Tokyo: Hakusuisha.
1977 "Chishiki shakaigaku no kotohajime" (Early days of the sociology of knowledge in Japan). Japanese Sociological Review 28(2):142–144.

Tanaka, Seisuke
  1965 "Kagakuteki shakaishugi-kyōsanshugi no mondai o megutte" (On the problems of scientific socialism-communism) Pp. 70–132 in S. Tanaka (ed.), Shakai-Ishikiron. Tokyo: Aoki Shoten.

Tanaka, Yasumasa
  1967 "Ideorogī kenkyū no hōhōteki kiso" (The methodological foundations of the study of ideology). Japanese Sociological Review 18(1):20–40.
  1971 "Gendai shakai to ideorogī" (Modern society and ideology). Shisō 570:60–80.
  1976 "Shakai-ishiki kenkyū no genjitsuteki kadai" (Practical problems in the study of shakai-ishiki). Pp. 203–253 in M. Mita (ed.), Shakai-Ishikiron. Tokyo: Tokyo Daigaku Shuppankai.
  1978 Shakai-Ishiki no Riron (A Theory of Shakai-Ishiki). Tokyo: Keisō Shobō.

Tōkei Sūri Kenkyūjo (Institute for Mathematical Statistics)
  1961 Nihonjin no Kokuminsei (The National Character of the Japanese People). Tokyo: Shiseidō.
  1970 Daini Nihonjin no Kokuminsei (The National Character of the Japanese People, 2nd ed). Tokyo: Shiseidō.

Yamagishi, Toshio
  1979 "Konjōshugi: 'Ore ni Tsuitekoi' no naiyō bunseki" (Secular asceticism in contemporary Japanese society: A content analysis of 'Follow Me''). Hitotsubashi Ronsō 81(2):79–95.

Yamazaki, Tatsuhiko
  1977 "Shakai-ishiki kenkyū no seikaku to tenkai" (The nature and the development of the study of shakai-ishiki). Japanese Sociological Review 28(2):103–120.

Yasuda, Saburo
  1970 "Shitsuteki dēta no bunseki to sūryōteki bunseki" (Analysis of qualitative data and quantitative methods). Japanese Sociological Review 21(2):78–85.

English Publications

Adler, Franz
  1957 "The range of sociology of knowledge." Pp. 396–423 in Howard Becker and Alvin Boskoff (eds.), Modern Sociological Theory in Continuity and Change. New York: Dryden Press.

Becker, Howard and Helmut Otto Dahlke
  1942 "Max Scheler's sociology of knowledge." Philosophical and Phenomenological Research 2(3):309–332.

Berger, Peter L. and Thomas Luckmann
  1966 The Social Construction of Reality: A Treatise in the Sociology of Knowledge. New York: Doubleday and Co.

Chall, Leo P.
  1958 "The sociology of knowledge." Pp. 286–303 in Joseph S. Roucek (ed.), Contemporary Sociology. New York: Philosophical Library.

Child, Arthur
  1941a "The problem of imputation in the sociology of knowledge." Ethics 51(2):200–219.
  1941b "The theoretical possibility of the sociology of knowledge." Ethics 51(4):392–418.
  1942 "The existential determination of thought." Ethics 52(2):153–185.
  1947 "The problem of truth in the sociology of knowledge." Ethics 58(1):18–34.

Coser, Lewis A.
  1968 "Sociology of knowledge." Pp. 428–434 in D. Shills (ed.), International Encyclopedia of the Social Sciences, Vol. 8. New York: The Macmillan Co. & The Free Press.

Curtis, James E. and John W. Petras
  1970 "Introduction." Pp. 1–85 in J. Curtis and J. Petras (eds.), The Sociology of Knowledge. London: Gerald Duckworth.
  1972 "The sociology of knowledge in American sociology: Origins, development, and future." Kansas Journal of Sociology 8(2):181–204.

Dahlke, Helmut Otto
  1940 "The sociology of knowledge." Pp. 64–89 in Harry Elmer Barnes, Howard Becker, and Frances Bennett Becker (eds.), Contemporary Social Theory. New York: Appleton-Century.

DeGre, Gerald
  1941 "The sociology of knowledge and the problem of truth." Journal of the History of Ideas 2(1):110–115.

Fairbank, John K., Edwin O. Reischauer, and Albert M. Craig
  1965 East Asia: The Modern Transformation. Boston: Houghton Mifflin.

Hamaguchi, Esyun
  1965 "A bibliographic overview in (sic) the postwar studies of Japanese culture." Psychologia 8:50–62.

Hinshaw, Virgil G., Jr.
  1948 "Epistemological relativism and the sociology of knowledge." Philosophy of Science 15(1):4–10.

Mannheim, Karl
  1946 Ideology and Utopia: An Introduction to the Sociology of Knowledge (with a preface by Louis Wirth). New York: Harcourt, Brace and Co.
  1952 Essays on the Sociology of Knowledge (edited by Paul Kegskemeti). New York: Oxford University Press.

Merton, Robert K.
  1945 "The sociology of knowledge." Pp. 366–405 in George Gurvitch and Wilbert E. Moore (eds.), Twentieth Century Sociology. New York: Philosophical Library.
  1968 Social Theory and Social Structure, Enlarged Edition. New York: Free Press.

Stark, Werner
  1958 The Sociology of Knowledge: An Essay in Aid of a Deeper Understanding of the History of Ideas. London: Routledge & Kegan Paul.

Steiner, Jesse F. and Kenneth K. Morita
  1956 "Present trends in Japanese sociology." Sociology and Social Research 41(2):87–92.

Wagatsuma, Hiroshi
    1969 "Major trends in social psychology in
          Japan." American Behavioral Scientist
          12:36–45.
Wolff, Kurt H.
    1967 "The sociology of knowledge in the United
          States of America." Current Sociology
          15(1):1–56.

    1971 "Introduction." Pp. xi–cxl in K. Wolff
          (ed.), From Karl Mannheim. New York:
          Oxford University Press.
    1975 "Introduction to fifty years of 'sociology of
          knowledge.'" Cultural Hermeneutics
          3:1–6.

MYTH AND SYMBOL AMONG THE NACIREMA TSIGOLOICOS: A FRAGMENT*

ROBERT ALUN JONES

University of Illinois–Urbana

[*Editor: The manuscript presented below was discovered almost inadvertently while doing archival research on Durkheim's* Elementary Forms of the Religious Life. *The document is clearly fragmentary and its authorship is indeterminate; but it is possible to draw some general inferences concerning authorship and date of composition from internal evidence alone. The subject under examination, for example, seems to be an elite clan of the Nacirema, that strange North American tribe whose exotic body ritual was classically explored by Horace Miner in 1956, and whose broader institutions have more recently been treated by Spradley and Rynkiewich (1975). The fact that these sources are not cited, however, together with the egregious and repetitive use of archaic masculine pronouns, and the patent absurdity of the notion that such bizarre mythological systems could have survived long into the twentieth century, clearly suggest a much earlier date of composition. Considering the evidence of subject matter and theoretical perspective, as well as the circumstances under which it was discovered, one is tempted to attribute the essay to an early member of the Durkheim School—possibly Hubert or Mauss. Indeed, the literary style, the extensive and knowledgeable comparisons with the Australian data and, most strikingly, the remarkable similarity and even identity of certain passages herein to those of* Les Formes élémentaires *suggest that this may have come from the pen of the master himself.*[1] *These suspicions entered, however, I felt it best to comply with strict editorial practices and publish the essay in its original, unexpurgated form.—R.A.J.*]

. . . The Tsigoloicos are a large, decentralized clan distributed throughout that geographical area occupied by the Nacirema. Though nomadic in disposition, the group has recently accepted more permanent settlement as a consequence of gradual exhaustion of natural resources. The clan has a relatively short history, and has only recently claimed elite status and conceived elaborate rituals whereby this is conferred upon new members. It is thus difficult to classify the Tsigoloicos in either of the two great Nacirema phratries (i.e., the Secneics and the Seitinamuh), a fact which probably explains the disdain in which they are held by the more firmly established clans of each.

---

[1] So stunning are these parallels that I have inserted page references, in brackets [], to the relevant passages in the English translation of The Elementary Forms of the Religious Life (1915). Otherwise the essay is presented without interpolations.—R.A.J.

The life of the Tsigoloicos seems to pass alternately through the two distinct phases observed among the central Australian tribes by Spencer and Gillen (1904). In the first phase, economic activity is indeed the preponderating one, and the Tsigoloicos limit themselves to the two primary occupations for which they are rewarded—i.e., the production and exchange of ritual inscriptions, and the preparation of neophytes for initiation into the clan. These occupations are typically pursued with only mediocre intensity and the dispersed condition of the clan results in a life which is uniform, languishing, and dull [EF, p. 246]. Periodically, however, the Tsigoloicos come together at a designated location to participate in a ceremony remarkably similar to the Arunta corrobori. Here, on the contrary, the reason and will of the primitive give way to his passional faculties, and he is caught up in the collective effervescence of communal life [EF, pp. 246, 258]. And while the ostensive function of such gatherings remains the exchange of ritual inscriptions, it is clear that their latent function is to reaffirm the solidarity of the clan—i.e., to give it a sense of itself and to reassure its members that it does, in fact, exist [EF, p. 257].

Our interest here, however, is in the religious institutions of the Tsigoloicos[2] and especially in the bizarre mythologies which underly them. These myths apparently grow out of the daily, profane activities of the Tsigoloicos themselves. As indicated above, each clan member is expected to periodically produce and exchange certain ritual inscriptions. For the most part, these inscriptions have little of the sacred about them; their utility is temporal and generally confined to the individual who produces them; and they are quickly deposited in the large vaults of Tsigoloicos temples, where they are soon forgotten.

A very few Tsigoloicos inscriptions, however, seem to have acquired and retained ·sacred status—i.e., they inspire collective sentiments of awe and respect,

and are set aside from profane inscriptions and treated with special precautions [EF, pp. 56, 301]. The Tsigoloicos themselves appear confused over how this sacredness came into existence; for these are typically very old inscriptions, written in oblique, turgid styles and even strange languages (ironically, the inscription seems to gain in dignity as its meaning is more obscure). As a result, the Tsigoloicos maintain a small class of priests, called *stsiroeht,* whose functions include the interpretation of sacred inscriptions and concoction of fantastic myths to account for their special powers. The result is a set of representations which express the nature of these sacred things and the relations which they sustain, either with each other, or with profane things [EF, p. 56]. At the same time, the Tsigoloicos have established rites which prescribe how a man should comport himself in the presence of these sacred objects [EF, p. 56].

THE SACRED BELIEFS

In order to account for the sacred inscriptions (or, more accurately, the sacred aspect of these inscriptions), the Tsigoloicos postulate the existence of mythical beings roughly comparable to the "civilizing heroes" of the Kurnai and Euahlayi [EF, pp. 328–329]. Originally, these beings are said to have been real men, the original members of the clan and ancestors of those in the present [EF, p. 328]. They possessed limited, if exceptional, powers [EF, pp. 310–311]; and, like all Tsigoloicos, they possessed souls and produced their ritual inscriptions to attest to this fact.

Upon death, however, these beings ceased being mere souls and acquired all the attributes of truly spiritual forces. The soul, of course, is merely a personality bound to a determined organism, and only occasionally succeeds in influencing outside objects during its terrestrial life; the spirit, on the contrary, has a more extended sphere of action and leads a virtually independent existence in free space [EF, p. 309]. The ancestral spirits of the Tsigoloicos are thus said to converge, diverge, influence, and generally act upon one another over the greatest distances in

---

[2] More recently, of course, these institutions have been treated in detail by Dynes (1974) and Friedrichs (1970).—R.A.J.

time and space. The mythical aspect of this belief is confirmed by the fact that, unlike mere mortals, these ancestral spirits are often said to have communicated with other spirits of whom they were not (indeed, could not have been) aware in their earthly existence.

A second divine power attributed to these spirits is one of exceptional prescience or clairvoyance. Certain spirits are thus said to have "anticipated" certain passages which appear in only the most recent Tsigoloicos inscriptions; and some are even held to have contributed substantial offerings to subclans of the Tsigoloicos not then even in existence.

Finally, a few ancestral spirits (e.g., Xram, Rebew, Miehkrud) are endowed with extraordinary creative powers. They are called the "founders" [EF, p. 320] or "fathers" [EF, pp. 162, 324] of the clan, the spirits who gave the clan its present form [EF, p. 282]. Like Putiaputia among the Arunta, they are said to have taught men the way of making inscriptions and using them ritually [EF, pp. 320–321]; and to the inscriptions of Xram, Rebew, or Miehkrud themselves, the stsiroeht ascribe the most miraculous powers. A very few of these inscriptions are sometimes said to embody all the sacredness diffused throughout the entire clan [EF, pp. 313–314].

The reader should not conclude, however, that the existence of these powerful spirits implies a state of utter dependence upon the part of the Tsigoloicos themselves, or, still less, that their efficacy is limited to the assistance of the individual clan member. Concerning the latter, of course, it is true that the belief has an individualistic aspect, for the Tsigoloicos ancestral spirit performs functions comparable to those of the Roman protecting genius [EF, pp. 61, 311, 317]. The neophyte is said to choose his own protecting spirit(s) during the extended initiation rite; thereafter, the spirit aids the individual in the hunt and warns of possible dangers [EF, pp. 162–163]. When in a struggle with another individual, the utterance of the name of the ancestral spirit (or, still more, the incantation of a passage from his sacred inscription) bears much power [EF, p. 143]; and the fate of the individual and his patron are closely joined—i.e., an injury done to the latter is taken as a blow to the former [EF, p. 315].

A further examination, however, suggests that the real significance of the ancestral spirit lies in its collective, rather than merely individual, functions. While the neophyte selects his own spirit, for example, this choice is made from the official, and quite limited, list of the clan [EF, p. 61]; and the range of possible alternatives is still further circumscribed by the largely fortuitous confinement of the neophyte at one or another of the Tsigoloicos temples. Furthermore, it is the clan which teaches the individual what these personal gods are, what their function is, how he should enter into relations with them, and how he should honor them [EF, pp. 61–62]. Finally, the individual's actions toward the spirit seem generated less by sentiments of fear or self-interest than by those of respect and moral obligation [EF, pp. 218–219].

This collective aspect of the ancestral spirit performs valuable functions for the Tsigoloicos. First, it is clear that clan members recognize the essential duties of kinship—e.g., aid, vengeance, mourning, vendetta, etc.—toward one another [EF, pp. 122–123; 218–219]. Other than the fact that they have the same name [EF, p. 122], however, there appears to be little support for such sentiments—e.g., they are not tied together by blood [EF, p. 122], resist any centralized authority [EF, p. 265], are nomadic by disposition [EF, p. 265], and remain geographically dispersed [EF, pp. 123, 265]. The postulation of ancestral spirits, however, affirms the sense of familial solidarity. Their unusual powers of conception, convergence, influence, etc., create genealogical connections between past and present members of the clan [EF, p. 157]; their immortality accounts for and affirms the survival of the clan beyond the ephemeral life of its members [EF, pp. 218, 304]; and since all clan members are now conceived as relatives, even the lowliest Tsigoloicos partakes of something of the sacred [EF, p. 159].

The ancestral spirit, therefore, is above all a collective representation—i.e., a symbol. As such, it both expresses and

maintains the clan's sense of social unity [*EF*, pp. 262–265]. This accounts for the curious relations which persist between the spirits and their descendants; for despite the extraordinary powers of the former, these relations are not those of superior to inferior [*EF*, p. 53]. Clan members are not always in a state of awe in the presence of sacred inscriptions; on the contrary, they frequently criticize their errors and inconsistencies, and there is open skepticism regarding any utilitarian value they might possess. The relationship is rather one of interdependence; for while the spirits clearly exercise an important function for the clan, it is equally clear that without numerous ritual observances (see below) the spirits themselves would lose their powers and die [*EF*, pp. 53, 386–387]. In fact, the sacred character of the ancestral spirits and their inscriptions owes little to their intrinsic properties [*EF*, p. 261]; for as we have seen, a strictly empirical examination suggests that the inscriptions are replete with errors, and the spirits themselves were originally but mortal men. Their sacred character, therefore, *is added to them* out of the need to create and maintain the solidarity of the clan [*EF*, p. 261].

This notion of ancestral spirits as collective representations, however, provides a testable hypothesis; for if the recent discussion of the role of such representations is correct,[3] the conception of these spirits held by the Tsigoloicos should systematically reflect the social organization of the clan. And indeed, this appears to be the case. The most recent stage of clan organization, for example, reveals a gradual fragmentation into relatively autonomous subclans which compete with one another over the sacredness (mana) diffused throughout the clan. The mythological classifications of the past appear to reflect this state [*EF*, p. 169]. Since each sub-clan needs to distinguish and individualize itself, it requires a special symbol for self-expression [*EF*, pp.

176–177]. The result is competition over the most powerful spirits; thus, a substantial number of the inscriptions of the *stsiroeht* are dedicated to demonstrating that one or another of these spirits was originally a number of that particular member's sub-clan. Where this fails, previously obscure ancestors are granted new, sacred status. And finally, wherever the identification of spirit with sub-clan is clear, there is an active debate over the relative powers of the ancestors—one into which the ancestors themselves, despite their previous ignorance of one another, are said to enter.

Under these conditions, it is not surprising that the mythology of the Tsigoloicos—i.e., the number of ancestral spirits, their classification, mutual relations, relative powers, etc.—is subject to constant change; for it is but a reflection of the changing social organization of the clan. Indeed, this condition extends even to those ancestors regarded as harmful or evil spirits. The existence of such spirits (e.g., Recneps) is postulated to account for the permanent evils which the clan has to suffer [*EF*, p. 318]. No sub-clan claims them for their own; there is thus no one who represents them, and they are without human posterity. When, judging from certain signs, it is believed that a child is the result of their work, it is put to death [*EF*, p. 319]. They are forbidden proximity or connection with their beneficent counterparts, and contact with the latter is regarded as the worst of profanations [*EF*, p. 456]. The significant irony is that, as has frequently happened, an impure or evil power can become a holy or guardian power; all that is required is the simple modification of external circumstances [*EF*, pp. 457–458].

THE SACRED RITES

The Tsigoloicos also maintain a set of ritual practices which prescribe how a man should comport himself in the presence of sacred things (inscriptions, spirits, or other members of the clan). These rites include aspects which are both negative (e.g., initiation rites) and positive (e.g., imitative, sacrificial, and commemorative rites) [*EF*, p. 337].

---

[3] This comment, of course, suggests considerable recent acquaintance with Durkheim's views, and thus supports the speculation concerning authorship entered above. Once again, however, these inferences are hardly conclusive.—R.A.J.

The negative cult arises from the fact that, by definition, a sacred thing is *separated* from its profane counterpart—i.e., there is a break in continuity between them. There emerges a whole set of rites whose object is to realize this state of separation and prevent confusion between the two orders of things [*EF*, pp. 337–338]. As such, these rites take the form of interdictions which impose abstinences upon clan members. If, for example, a clan member confuses a sacred inscription with its profane counterpart, or an evil with a guardian spirit, material disorders are brought about automatically; the guilty man suffers, and this is regarded as judgment on his act [*EF*, pp. 338–339]. Similarly, there are interdictions which express the inequalities and incompatibilities among sacred things [*EF*, p. 340]. The old men of the clan, for example, are said to possess more of the sacred, and things forbidden to ordinary people are permissable for them; and younger clan members (and especially women), though fully initiated, are said to be less sacred and treated accordingly [*EF*, p. 161].

The initiation ceremony, however, marks the most dramatic expression of this negative aspect of the cult. Due to the barrier which separates the sacred from the profane, a man cannot enter into intimate relations with sacred things except after ridding himself of all that is profane in him [*EF*, p. 348]. This process begins when the neophyte enters a Tsigoloicos temple, whereby he more or less withdraws from the profane world; the subsequent trials, which sometimes take many years, involve a series of severe ritual abstinences, including the eating of impure foods, fasting, loss of sleep, infrequent bathing, etc.—all under the eyes of the old men of the clan, who serve him as godfathers [see the nearly identical list of ritual abstinences in the *EF*, p. 349]. The result of these interdictions is to bring about a radical change of condition in the neophyte. Excluded from the cult before these negative rites, he is now admitted to its society, for he has acquired a sacred character [see *EF*, p. 350].

Such practices, incidently, are justified as probationary—i.e., they ostensibly prove the value of the neophyte and show that he is worthy of admission to religious society. A close examination, however, shows that this probationary aspect is mere legitimation. The rite possesses efficacy not because it alters the intrinsic nature of its subject, but because the clan attaches sacred qualities to those individuals who have survived it [*EF*, p. 353].

At the completion of the initiation ritual, the individual is introduced into the more positive aspects of the religious life of the clan. The rites of particular interest here include those of imitation, sacrifice, and commemoration. The imitative rite consists of the periodic production of inscriptions which emulate those of the temple godfathers, or, still more desirable, those of the ancestral spirits one has acquired during the initiation. The formula upon which this rite is founded is that of "like produces like"—i.e., the representation of a being or condition produces this being or condition [*EF*, p. 398]. This representation does not imply exact imitation, however, for severe punishment awaits the author of too literal a transcription; the goal is rather representation and suggestion—i.e., to create an image which recalls the inscriptions of the old men of the clan or the ancestoral spirit [*EF*, p. 400]. The successful inscription, however, acquires a sacredness similar to, though by no means as great as, those of their models; for these representations are collective symbols as well. By producing them, clan members mutually show one another that they are all members of the same moral community and they become conscious of the bonds of kinship uniting them [*EF*, p. 400].

For such imitative rites to have their effect, however, the inscriptions of individual clan members must be available to the Tsigoloicos as a whole. The individual thus participates in a sacrificial rite, whereby he periodically offers his inscription to the clan and renounces any subsequent claims over it [*EF*, pp. 377, 385–386]. Those oblations, however, by no means exhaust the ritual of sacrifice, for the sacrifice is an act of communion as well as oblation [*EF*, pp. 378, 384]. In addition to offering his own inscriptions to the spirits of the clan, therefore, the clan,

member must also incorporate the inscriptions of others. In doing so, the individual partakes of whatever sacredness exists in these inscriptions; indeed, the Tsigoloicos believe that an individual cannot retain his position in the clan unless he thus periodically revivifies the sacred principle that is within him [*EF*, pp. 378–379].

Finally, like the Warramunga [*EF*, pp. 415–416] the Tsigoloicos engage in a peculiar rite whose only apparent function is the representation of the mythological history of the clan. In this ritual, a few members of the clan produce inscriptions which do nothing more than describe the extraordinary feats of the ancestral spirits. Occasionally, of course, the rite is justified and defended on the basis of its imputed practical effects; but, as in the initiation, these appear to be wholly secondary and bear no relation to the liturgical importance attributed to these rites [*EF*, p. 240]. The sole object of the rite thus appears limited to rendering the mythical past of the clan present in the mind [*EF*, p. 419]. But the efficacy of the rite arises precisely from the fact that this mythical past is nothing more than a system of beliefs common to the clan—i.e., a collective memory; the performance of the ritual thus sustains the vitality of these beliefs, prevents their being effaced from memory, and rekindles the most essential elements of the collective consciousness [*EF*, pp. 419–420].

CONCLUSION

The primitive beliefs and rites of the Tsigoloicos thus appear somewhat bizarre and disconcerting, especially to the modern observer. At least two cautionary remarks, however, should be entered. First,

however crude, mistaken, and unsophisticated their mythology may seem, it should be recalled that this construction of a mythical past is but an earlier form of the modern practice of historical scholarship. The conception of previous ancestral spirits is an ambitious, if fore-shortened, attempt to place events accurately in their historical time; and the extraordinary powers attributed to these spirits is undoubtedly a primitive approximation of our own, though far more sophisticated, conception of historical causation [*EF*, p. 21].

Second, however erroneous and mistaken the Tsigoloicos mythology may be, it should not be conceived as a total fiction. Indeed, it is an essential postulate of sociology that a human institution cannot rest upon an error and a lie. While the letter of such beliefs appears misconceived, therefore, we may assume that these most fantastic rites and barbarous myths but translate real social needs [see *EF*, pp. 14–15; here the manuscript ends].

REFERENCES

Durkheim, Emile
  1915 The Elementary Forms of the Religious Life. New York: Free Press.
Dynes, Russell R.
  1974 "Sociology as a religious movement: Thoughts on the institutionalization of sociology in the United States." The American Sociologist 9:169–176.
Friedrichs, Robert W.
  1970 A Sociology of Sociology. New York: Free Press.
Miner, Horace
  1956 "Body ritual among the Nacirema." The American Anthropologist 58:503–507.
Spencer, Baldwin and F. J. Gillen
  1904 The Northern Tribes of Central Australia. London: MacMillan & Co., Ltd.
Spradley, James P. and Michael A. Rynkiewich
  1975 The Nacirema. Boston: Little, Brown and Company.

JOURNEY TO FORTRAN: A YANKEE WAY OF KNOWLEDGE*

Marcos Montezuma, Ph.D.

I am an old man of 70 winters, a Yaqui Indian who has lived all his life in the great Mexican desert of Sonora. In 1976, while doing research on the drugs used by the white men of the North to induce states of nonordinary reality, I heard of a sorcerer said to be adept in the use of the powerful, mind-altering drug Valium *(diazepam)*, and I resolved to seek him out.

I packed my belongings on the back of my burro and travelled west and north for many days. After I had journeyed hundreds of miles, I came to a large city on the ocean shore, a city far larger than any I had seen before. I travelled westward through that sprawling city until I came to a great university, and I rode my burro up to a brick, L-shaped building near its center. Inside, I espied a young man sitting at what seemed to be some kind of typewriter. His skin was pallid, as if he seldom ventured into the light of the sun. My long journey was over—I was face to face with the sorcerer wan Don.

"Wan Don," I entreated, "I have travelled many miles to learn the nature of reality. But I almost despair of doing so.

For I know that each of us has his own separate reality. I am an old man, and I fear that there are not years left in my life for me to learn all these different realities."

"Folklore," replied wan Don. "There is only one reality. I will take you, Marcos, as my apprentice, and teach you how to know that reality."

Trembling, I asked, "wan Don, in order to know reality, must one chew the peyote button?"

"No, Marcitos," he said, "one must push the computer button."

He led me to a room in the bowels of the building, a room that was filled with strange machines I had never seen before. He took some rectangular cardboard objects with tiny holes in them and placed them in the machine. Immediately a whirring, clicking, and clattering began, such as I had never heard before.

In terror I cried out, "What is happening, wan Don? Are you stopping the world?"

"No," he replied, "I'm starting the run."

Coming soon: the next volume of the series, where Marcos struggles to "see" reality as wan Don does and still retain his sense of Yuma.

ON THE IMPORTANCE OF THE SOCIOLOGY OF SCIENCE
TO THE GOALS OF GENERAL EDUCATION*

JOEL PETER EIGEN

Franklin and Marshall College

One hears in current curricular discussions renewed interest in affirming the goals of general education, particularly that of instilling intellectual curiosity. The sociology of science offers to undergraduate education both an analytical framework and a body of literature with which to further these goals. This article highlights several themes in the sociology of science and examines how, in applying these perspectives to two areas in undergraduate sociology, students can be led to question the "received word" by seeking out the relationship between the production of scientific knowledge and the socio-political context in which scientific ideas are pursued. Suggestions for extending this perspective to sociobiology are discussed.

The goal of general education is to foster intellectual curiosity. One often uses the term "lifelong learner" to signify the consummate achievement of a liberal arts education. Such an education seeks not only to familiarize students with major substantive areas of knowledge—although it does strive for that—but also attempts to instill an appreciation for the structure of ideas and argument.

Within a liberal arts curriculum, general education aims at "liberating the student from uncritical loyalties, prejudice, and provincialism" (Carnegie Commission, 1977:154). To be "liberated," to be able to question previously held assumptions, one must be sensitive to the way knowledge is received. Only in developing a good critical sense, a "healthy skepticism," can there be a basis for arriving at evaluative statements comparing the relative worth between and among ideas. Those college curricula truly sensitive to these goals have the best chance to produce inquisitive, curious students, i.e., lifelong learners.[1]

One sees in the re-emergence of core curriculum, evidence of a certain anxiety on the part of today's educators that the above goals are not being met. Harvard's 1978 "Report on the Core Curriculum" has received a good deal of publicity, but it is by no means an isolated attempt to translate purpose into substance. One hears a lively debate in colleges and universities of all sizes over the minimum acceptable standard to be attained by each graduate. The standard would, in part, acquaint students with major areas of knowledge and the mode of inquiry characteristic of these areas. And there is concurrently a desire to expose students to modes of analysis and model building which, regardless of subject area, are essential if one is to make value judgments. This article will examine how a major area in sociology, the sociology of science, can be used to further the goals of general education through the question it raises in looking at scientific activity as a cultural process.

[1] Although intuitively sound, there is little supportive evidence that exposure to the principles of general education does indeed produce lifelong learners. Astin, after looking at several longitudinal studies, concluded that "information on the perma-nence of impact of different types of colleges and college experiences is limited primarily because such studies have focused on one outcome (income) and used one measure of college characteristics (selectivity or quality)" (1977:209). When studies have looked at college attendance and 25 year follow-up performance on achievement tests it is unclear whether superior achievement of the college trained is due to college experience, or whether their subjects had chosen to continue their education because college merely reinforced curiosity and reading habits gained earlier.

SEVERAL THEMES IN THE
SOCIOLOGY OF SCIENCE

One way to nurture the curiosity so often referred to as the cornerstone of liberal learning is to challenge students to seek out relationships between changing scientific theory and changing social structure. This entails a reconsideration of the scientist as "passive observer of nature's immutable laws," and though this might strike some readers as naive positivism, few undergraduate instructors have not had to face the perception of scientists as "fact finders." Rarely are students in the natural (or social) sciences asked to consider the context in which scientific ideas are accepted or rejected, or the existence of a normative structure to scientific work, or further, the extent of social responsibility scientists bear for the consequences of their work. Are these concerns only of interest after the data are in, or might they actually structure the inquiry and affect the interpretation of empirical findings?

The first issue looks to the forum in which scientific discoveries are received and discussed. Some sort of evaluation is eventually made that a discovery or set of observations constitutes "valid knowledge," but how is this assessment made? Although the unadorned positivist stance would lead one to suspect that scientists only "read" nature in some passive manner, the history of science literature provides rich instances of institutional and national boundaries which have conditioned the reception and diffusion of scientific ideas. Robert Merton (1968) is particularly interested in the tendency within scientific circles to deny the historical import of the modern day discovery. He refers to the search for all earlier forms of the contemporary idea or discovery as adumbrationism, and suggests that one reason for this propensity may simply be chauvinism, a desire to claim scientific heritage along national boundaries and to discount any scientific discovery save by "one's own." In this regard, one could argue that Darwin's ideas were not taught in France until 1871 because of the French preference for their own evolutionist, Jean Baptiste Lamarck. Quite simply, no com-pelling reason existed for scrapping Lamarck's ideas in favor of the newer (and British) Darwin. Lamarck was eventually replaced when the French realized how hopelessly antiquated their scientific academies were. This startling discovery came at the hands of the "backward" Prussians, whose advanced technology and knowledge of munitions successfully triumphed over the more civilized French in 1871. The military defeat appears to have precipitated the first curricular review of French academic science since the founding of the Ecole Polytechnique nearly a century before. As the example illustrates, paying attention to institutional allegiances and recognizing the intrusion of historical events on scientific activity can highlight the temporal and cultural aspects of scientific works and alert students to the critical nature of the context in which scientific work is received and discussed.[2]

The positivist stance which leads to "scientist as fact finder" also gives rise to the conception of science as serendipitous discovery. To counter this perception, sociologists of science (Merton, 1970; Mulkay, 1976) have drawn attention to a probable normative structure which guides research activity. Although there seems to be some debate regarding exactly which norms characterize scientific work, there appears to be a consensus that normative models do indeed structure inquiry and give direction and perhaps interpretation to laboratory observations.[3]

---

[2] For related discussions on the differences between national and temporal boundaries separating scientific communities, see Johnston (1976) and MacLeod (1977).

[3] According to Merton (1970) the ethos of modern science embraces the four institutional norms of universalism (truth claims to be based on merit, alone), communism (scientists enjoy collective rights in knowledge and share their results), disinterestedness (emotional neutrality), and organized skepticism. The stress one sees here on rationality in the laboratory can be traced to Merton's description of the historical absorption of Puritan sensibilities into scientific research. The Puritan ethic which on the outside emphasized direct communion with God through nature, fostered according to Merton, scientific values of empiricism, instrumentalism, and utilitarianism. The existence of a set of counternorms has recently been argued (Mitroff in Mulkay, 1976) in which commitment, secrecy, and

Scientific work is purposeful, goal directed activity, and whether one chooses to cite institutional norms embracing value neutrality and universalism or the so-called counternorms of secrecy and particularism, the existence of prescriptive norms that structure scientific inquiry serves to remind students that scientists belong to social groups whose members publicly share common values—objectivity, the sanctity of the scientific method—and whose work may similarly be guided by shared ideological beliefs—for example, positivism or Marxism (Mulkay, 1979). As undergraduates are asked to probe questions of group commitment to both a particular scientific model and to the perception of science as "objective" and emotionally neutral, they are led to an appreciation of scientists as social actors whose work has been described as a "socially constructed reality."[4]

In addition to their membership in social groups committed to this or that "model of reality," scientists also enjoy an increasingly influential role in the political process by virtue of their role as government consultants and principal investigators in federally funded research. The need to strike a balance between the canons of scientific research and social policy choices likely to involve partisan interests, constitutes another area of inquiry for science and society: the social accountability of scientific research. As Joseph Haberer points out, greater political involvement and an enhanced role in formulating public policy carry a responsibility beyond questions of methodological adequacy and competent judgment. Haberer wonders: "Who is to be held re-

---

particularism—selecting work of those scientists whose work is judged to be reliable—might also serve as normative guidelines for scientists. These two sets of norms are not necessarily adversarial, but rather they may be descriptive of different stages in the research enterprise when one set of norms is more binding than the other.

[4] The effort to define science as socially constructed reality represents an attempt by sociologists of science to extend their focus into epistemological concerns traditionally within the province of the sociology of knowledge. According to Johnston, the initial boundary between these two areas had been maintained by Robert Merton and Joseph Ben-David who both felt that the actual production of scientific knowledge was not open to sociological analysis. For them, the content of scientific work was a search for truth; "knowledge is generated by an autonomous intellectual tradition" (1976:196). For a related discussion on the positivist approach to physical science, see Krohn (1977).

For other sociologists of science, however, the content of scientific work is not "autonomous," but rather socially constructed, particularly as scientists endeavor to discover what is real. Their position closely resembles the perspective of the sociology of knowledge which seeks the relationship between knowledge and social existence. In sketching this special subfield, Mannheim (1936) looked to the different ways an object could present itself and in turn be perceived by an observer according to varying social settings, and the varying social position of the observer. It is the experiential factor which Mannheim finds so compelling: the experience of social life, of social groups, of competition and social status which affect one's perception. No absolute truth here; truth does not remain constant over time, but

evolves along with the processes of historical change. A "sphere of truth in itself" is rejected by Mannheim as both "disruptive and unjustifiable" (1936:305).

It is this conception of knowledge as mediated by human thought, as conditioned and constrained by institutional and national boundaries, that emerges as a potent theme in recent work in the sociology of science. MacLeod contends that social and contextual factors can influence the content of science; "methods and formulae, analogies and metaphors, become an expression of views of people at different times" (1977:174). As such, these views become embedded in the socialization of subsequent generations of scientists and help to fashion and buttress the ruling paradigm. Johnston (1976) notes that academic science is "consensual." The novice must learn to use accepted terms and "correct apparatus" which will ultimately affect his or her approach to their naturalistic studies. The contention, then, that knowledge is mediated through thought processes that might be contextually shaped and institutionally inspired places sociology of science, for these writers at least, on the threshold of the sociology of knowledge.

There still remains a considerable conceptual distance to be bridged between the two subspecialties. Invoking Mannheim's concern with social structure, existence, and thought does not immediately substantiate the claim that science is a "socially constructed reality." One still must seek out how the scientist's perception is affected by his or her social status, group commitments, and life experiences. Something beyond "science as ideological superstructure" is clearly called for. How do external inputs, if indeed the scientist recognizes them as external, come to condition observations made in the laboratory or in the field? As Johnson writes, there is a need for a "theory of mediation between socioeconomic factors and the cognitive aspects of social reality which constitute and condition science" (1976:199).

sponsible for a decision (on public policy), by whom, and on what grounds. . . .?" (1969:326). This is no less fitting a question for the physicist working with nuclear reactors than for the criminologist predicting delinquent careers. Will "society" ultimately be the richer for its greater understanding of natural (and social) phenomena? Should questions of accountability constrain the choice of research topics? Do they?

These three areas—variable reception of scientific ideas, normative structure, and social accountability—are representative of some of the concerns in the sociology of science literature; they are not meant to be exhaustive nor to limit the scope of classroom inquiry. They are meant to challenge those students whose view of science is one of an insular, culture-free pursuit. Related to the above concerns is an interest in seeking out relationships between the interpretation of empirical findings and the analogic or metaphorical reasoning that scientists employ to bring coherence to their observations. Often times, the metaphor has a social or cultural origin. The next section will explore how topics in the sociology of science can be used to help students analyze and evaluate the literature in two curricular areas in undergraduate sociology: the history of social theory and criminology.

HISTORY OF SOCIAL THEORY

Most students entering college have some familiarity, if only at the recognition level, with Charles Darwin's *On the Origin of Species*. Darwin's meticulous data collection and restraint in publicizing his hypothesis concerning the dynamics of evolution serve as a model of cool, dispassionate scientific research. But how are students to respond to the idea of "scientific research:" as an insulated, independent disciplinary tradition of the scientist, or as a cultural activity receiving inputs from the surrounding culture?

Darwin's work provides an excellent opportunity to explore the internalist/externalist debate which has played such a seminal role in the history of science. The internalist position is argued by those

historians (and scientists) who view the production and validation of scientific knowledge to be isolated, to a great extent, from cultural and non scientific factors (Kuhn, 1977). There is a limited role for external inputs in the posing of initial puzzles or enigmas which set the research in motion. But once graduate and other forms of specialized training produce subcultures of scientists concentrating on particular areas of study, it is the *internal problem generated by a particular paradigm* that prescribes the direction of scientific inquiry.[5] This is the oldest, and for many physical scientists, the model that most accurately depicts the progress of naturalistic observation; when it is applied to Darwin's work it yields the picture of deliberate, patient accumulation of evidence leading sequentially to the theoretical constructs of species variation and natural selection.

There is an alternative view to the way scientists go about their work. The externalist view contends that ideological, political, and economic changes affect all groups in a society, and one of these groups is, of course, scientists. External inputs are cited in the timing of scientific work, in the differential appeal of one or another speciality, and in the area of institutional reform—the opening up of new channels of communication and the opportunity for "cross-fertilization" (Kuhn, 1977). Merton's work (1970) on the relationship between Puritan values embracing empiricism and instrumentalism and the quantum leap in scientific activity in 16th and 17th century England is an example of the externalist approach.[6]

---

[5] Some writers have speculated that the internalist's steadfast avoidance of social context arguments may be motivated by a desire on the part of scientists to protect their claims to political neutrality and freedom from outside control (Johnston, 1976) or because of "deep seated fears of epistemological relativism" (MacLeod 1977:160).

[6] Mendelsohn has also pointed out the close correspondence between cultural shifts and new scientific directions in the 16th and 17th centuries:

No conspiracy theory of some special capitalist influence on the formation of the scientific way of knowing is necessary. The actors and their missions were drawn from the same segment of society and the same group conflicts and social forces for both the nascent scientist and the aspiring capitalist (1977:16).

The utility of separating inputs into "internal" and "external" categories has recently been criticized as both artificial and nonproductive. Johnston (1976) argues instead for a contextual model, one that recognizes the need to account for the purposive nature of scientific work. Goals, writes Johnston, are determined by the level of cognitive development; they are themselves part of the research process. When one considers scientific work as goal directed, social activity, one looks for a commitment that scientists make to a particular model of reality. This concern with externally inspired goals and internally evolving value commitments suggests that there may be a way to relate the social and economic surroundings to the way in which scientists approach the natural world. Just such an attempt to synthesize the social and economic context of 19th century England with the internally evolving disciplines of botany and biology makes Darwin's work such a fruitful area for sociological inquiry.

Considerations of time and space play a central role in the sociology of science. In terms of biological theory, Mulkay (1979) has noted that theories of evolution first took root in countries experiencing thriving capitalist expansion. One reason for this intellectual curiosity was that in exploring the New World for foreign markets, merchants brought back not only raw materials for production but also evidence of a multitude of varieties of plant and animal life. One needs to remember that Lyell, Darwin's mentor, has supported Cuvier's conclusion that there was a definite limit to variations around a mean (Young, 1971). The question prompted by these new discoveries was: "Why should such variation occur?"

Commercial sea-faring vessels often took on amateur naturalists to collect and bring back enigmatic variations found in the New World. Darwin, at the time of the now famous voyage of the Beagle, was just such an amateur (Bernal, 1954). A man of independent means and no fixed occupational prospects, he was in a position to take advantage of the opportunity for study and self-reflection provided by the long South American journey.

Gould (1977a) points out that Darwin's

social status might have played an even larger role in affording him the time and space needed to collect his evidence of the range of plant and bird variations. He sailed " . . . as (the captain's) companion, primarily to share his table at mealtime for every shipboard dinner during five long years" (1977:30a). Out of this friendship which was based, at least initially, on similarity of class background, Darwin secured the conditions necessary to inspect fully the wildlife of the Galapagos Islands. It was while making these observations that he was struck with the subtle variations in species and wondered why varieties of bird and plant life had grown to be so different from their ancestors on the mainland.

If nothing else, this story illustrates the importance of social class as a consideration in the history of science. How different would the science of biology be today if Darwin had been the offspring of a tradesman and not the son of a very wealthy physician. Darwin's personal riches gave him the freedom to pursue research without encumbrance. Since his various illnesses often permitted only two to three hours of fruitful work per day, any need to make an honest living would probably have shut him off from research entirely. We now learn that Darwin's social standing also played a crucial role at a turning point in his career. Fitzroy (the Captain), was far more interested in his mealtime companion's social graces than his competence in natural history (Gould, 1977a:31).

The influence of Britain's business sector on Darwin's work, however, went beyond the initial opportunity to observe species variation. Commercial breeders most likely provided him with the theoretical framework that was to serve as the conceptual basis for natural selection. This framework originated in the commercial practice of artificial selection: choosing for mating varieties which best met the needs of the breeders. Those varieties best "adapted" to the needs of the commercial community were selected for selling and reproduction; those forms deemed unsatisfactory were discarded. Selection thus came to signify "successful adaptation." The rewards that could accrue from proper "adaptation" were not lost on Darwin; "Hard Cash paid down over and over again is an excellent test of

inherited superiority" (Darwin in Mulkay, 1979:102).

It appears to be this very conscious human practice of selecting one variety over another which proved so central to Darwin's conception of the process of evolution in nature (Ruse, 1973). Because the sociology of science challenges students to look for informal, metaphorical reasoning in the laboratory, this particular metaphor of purposeful selection takes on special significance. As commercial breeders choose the more robust varieties of each species for "survival," so nature must "select" those forms (including human forms) best adapted to the rigors of the environment.[7]

But how to explain the origin of new types in nature? Darwin again drew on the formation of new breeds by artificial selection. First, he assumed that the potential for variability was constant; organic forms showed a tendency to vary when domesticated. Second, he isolated the elements of evolution: random variation, struggle for existence, and adaptive selection by the environment.[8] Of these three, he could produce only evidence of random variation. To persuade his scientific audience that new forms could originate in the struggle for survival among variations, he drew on one of the most controversial and widely debated social topics of his time: competition and favorable adaptation.

During the twenty years that Darwin spent collecting evidence of random variation, few writers could claim greater impact on European consciousness than Thomas Malthus (Young, 1969). His grim analysis of the inevitability of human misery and privation as positive checks on population growth provided a demographic model uniquely fitted to the debate about social struggle and adaptation.[9]

Because all organisms tended to vary, available food stuffs would inevitably be depleted in an effort to sustain all forms of existing life. The only checks on this population growth were disease and war. How then does one account for the survival of some variations?

Adaptation became the conceptual tool to explain selective survival. The organism varies in no determinate direction; no inherent species perfectibility was contended. Instead, Darwin argued that nature selects for certain characteristics (Greene, 1959). Earlier, Malthus and Herbert Spencer argued that those human forms best adapted to the rigors of competition had the greatest chance for survival (and did indeed survive!). Darwin was then able to depict nature as selecting those varieties with favorable adaptation in the wild. One needs to remember that Darwin had no direct evidence of this; artificial selection does not show " . . . either that natural selection exists, or that if it does exist, in what direction such selection points" (Ruse, 1973:103). He could specify neither the laws of variation nor the means by which variations were preserved. Darwin's task, according to Young was to "explain away the lack of evidence while repeatedly stressing the greater plausibility of his theory over that of special creation" (1969:469). What Darwin could rely on was an array of Victorian attitudes embracing cultural ideas of conflict, competitiveness, and domination which characterized mid-19th Century thought (Gale, 1972:342–343).

When Darwin invoked Malthusian principles to account for natural selection out of the struggle for survival, he also made his theory especially attractive to the business community. Those who sat at the top of the social and economic pyramid quoted both Malthus and Spencer as justification for a status quo, non interventionist State policy. Natural selection gave their arguments further strength; because we are all "put on trial," unfettered competition was the

---

[7] For a detailed discussion of artificial selection and heritable change in captivity, see Ruse (1973:102–107).

[8] Pre-Darwinian naturalists also recognized the existence of struggle, but did not grant it a primordial role in evolution. For a discussion of the different conceptions of struggle in nature predating Darwin's work see Gale (1972).

[9] Although the concept of struggle is central to his hypothesis, Malthus does not grant it a "creative"

role; no species perfection or qualitative improvement is achieved. Indeed Gale writes that Malthus ". . . discusses struggle within an anti-evolutionary context" (1972:337).

only way to ensure progress. Progress was perhaps the most powerful cultural theme in 19th Century Europe, and Darwin's conception of evolution fit perfectly with the existing social, economic, and political doctrine (Merz, 1923).

Students of the history and sociology of science are asked to pursue the origin and implications of a scientist's beginning assumptions. Does "survival of the fittest" allow for the reality of inherited privilege? Had Darwin not used a model of competition, might one see a different conception of "favorable adaptation?" Is it humankind's lot to sit passively by and merely observe the ineluctable progress of nature's "stern discipline," or can ameliorative measures be taken?

It is by probing these working assumptions that one is able to see science as "cultural inquiry," as an interpretive enterprise whose practitioners are conscious members of not only their own specialized subcultures but of the larger culture as well. As members of both worlds they construct their own sense of meaning, in this case, bestowing the legitimacy of "true knowledge" on that which is observable and "real."[10] One wonders, though, how much of this observation is conditioned through ideological commitments to one paradigm or another, and to

---

[10] This position has recently been explored by Overington who stresses the community-based standards of scientists in the evaluation (and production) of scientific knowledge. It is this "socially contextual and communitarian character of scientific knowledge which undermines the privileged character of that knowledge" (1979:5). By "privileged knowledge" Overington is referring to the claim that scientific knowledge is somehow "true knowledge," free from the contaminating influences of external inputs.

Overington pays particular attention to the degree of commitment scientists show in their work. As Kuhn has noted, scientists are recruited through a process of socialization which offers to the novice models of reality, standards for prediction, and judgments of adequacy. Students are trained in "convergent thinking" (Kuhn, 1977:226). As members of special subcultures, they have a sizeable investment in the current picture of social reality and devote their energy to making their paradigm fit with the available evidence. Having considered these factors, Overington asks whether science should continue to enjoy its "privileged knowledge" status, or whether sociologists of science should turn their attention to how scientific communities "construct what they accept as factual or known."

what extent " . . . social ideologies can adopt those parts of scientific knowledge which are acceptable to the interests of a class, or a system of other social beliefs. . . ." (MacLeod, 1977:169). In Darwin's case, his relationship to Britain's business community, his use of the metaphor of purposeful selection, and his reliance on Malthus, help the student to question the "privileged" nature of science as an insulated, "culture-free" pursuit.

CRIMINOLOGY

In addition to alerting students to recognize the effect of cultural themes in science, undergraduate sociology also can strive to utilize the range of analytic techniques presented by Thomas Kuhn in *The Structure of Scientific Revolutions* (1962). Concepts of paradigm, anomaly, and scientific revolution then can be applied to the question of how paradigmatic shifts occur in the history of natural and social sciences. A particularly compelling analysis of just such a shift in criminology has been offered by Robert Nye (1976).

Cesare Lombroso often is credited with being the father of scientific criminology. *L'Uomo Delinquente*, published in 1876, proferred a biological theory of crime causation which immediately caught the attention of the legal and scientific community. A physician specializing in cretenism and anthropometric measurements, Lombroso arrived at his theory in large part due to an autopsy he performed on the most notorious brigand of his day, Vilella. Lombroso found a depression on the occipital fossa which resembled the cranial formation of lower-order birds and rodents (Wolfgang, 1960). His conclusion was that criminals were human forms who had failed to evolve, they were "atavists," and as such were incapable of living in 19th Century civilized society.

Lombroso's use of evolutionary theory to fashion the first "scientific" theory of criminal behavior is a fitting example of the force and diffusion of scientific ideas far removed from their original formulation. The notion of the criminal's "failure to evolve" gained immediate currency in a scientific community already excited by

the theory of recapitulation (Gould, 1977b). Lombroso not only claimed that what one calls crime among "civilized adults" was actually normal behavior among animals, he further mentioned that one could find a justification for capital punishment in the demonstrated effectiveness of natural selection. Such was the spread of originally "internal" concepts of biological evolution.

Lombroso's work was part of the larger scientific tradition of positivism which had been given its initial focus by Comte and Spencer. According to the positivist's perspective, social behavior was the effect of antecedent causes located in the physical universe. Consequently the human species lost its link to divinity and appeared as a purely naturalistic phenomenon whose manner had been determined by so many external "causes."

In 1885, the fortunes of Lombroso and the "Italian School" were at their highest point. The First International Congress of Criminal Anthropology held in Rome witnessed the gathering of European delegates to pay homage to Lombroso and his celebrated students Enrico Ferri and Raffaele Garofalo. Included in this conference was a group of French social scientists. Most prominent in this latter group was Alexander Lacassange.

Lacassange presented the first serious threat to Lombroso's biological model. He warned against using such unproved words as atavism and Darwinism, asserting that it was absurd to associate "primitive anatomical characteristics with a pathological predisposition to crime" (Nye, 1976:339). The only recourse left to society was to construct institutions in which to "warehouse the degenerates," and throw away the key.

Historically, the conference drove a wedge between the French and Italian Schools of Criminology. The French School eventually triumphed, but why? Can one attribute the change to the accumulation of internal anomalies or to the weight of external pressures that would mold the environmental school of the 1890s. In seeking out the connections between the social origins of the reformers and the occupational structure of the French courts, students begin to recog-

nize that the role science is asked to play often is conditioned by social forces and this affects the type of questions one asks and consequently the content of the research.

For Lacassange and the other French social scientists, the crucial concern was social milieu. Not only did environment provide the setting for the germination of etiological criminal traits, it also served as an alternate force in evolution. The French, it will be remembered, showed a distaste for Darwin preferring instead his predecessor, Lamarck who first propounded the theory of the heritability of acquired characteristics.[11]

Lamarckian evolutionary thought held out to the social sciences the possibility of modifying the criminal's behavior. Adaptation was Lamarck's animating principle; not maladaptation and selection, but positive adjustment and perfection. A second "French source" for this environmental emphasis was Pasteur whose emphasis on culture as the focal point in the growth of bacteria also was adapted to the criminological debate. The microbe was the individual who only adopted criminal traits in the proper fermenting broth. This shift in focus to melieu and adaptive potential gave the French School a strong internalist argument against Lombroso; the fundamental assumptions which guided Lombroso's work were unwarranted and produced a "flawed" conception of criminal evolution. But the debates over evolution were still at a theoretical level; neither school could provide sufficient material evidence to substantiate its claims. There appears to have been no obvious, "internal" reason why the French should have predominated.

An externalist argument has been made

---

[11] Lamarck's theory is a behavioral approach to biological evolution; the mechanism of evolution was the organism's interaction with the environment (Stocking, 1968). Structural modifications to the organism accomplished through its lifetime could be passed on to the next generation, thus preserving these traits through reproduction. The implication for social behavior was quite apparent; culture also could be transmitted through the inheritance of acquired characteristics. There is then the possibility of improving the human species. Culture simply accumulated in one's blood and was preserved through inheritance.

by Robert Nye (1976). In his perceptive comparison of the structure of courtroom roles in Italy and France, one begins to see the true impact of cultural milieu on science. French legal practice differed from the Italian form in the critical area of psychiatric expert testimony. The Napoleonic legal code gave birth to legal medicine in France, officially allowing the psychiatrist into the courtroom as an expert witness. He was in the courtroom, however, only at the pleasure of the presiding judge. The psychiatrist's income and professional security therefore depended on a cooperative relationship with the presiding magistrate. It was in the nature of this dependency that one sees the attraction to a "soft" environmental causation theory, one that did not question the judge's sentencing prerogatives or role in guilt-fastening, but one that still allowed the expert to advise the bench.

In contrast, a theory embracing biological causation left relatively little for the judiciary to do. The trial itself easily could become a scientific forum replacing, as Ferri advocated, juries with psychiatrists. The judiciary quite rightly could fear the usurping role science might have on its own occupational niche (Nye, 1976:345). How best then to show the court's recognition of the importance of science without compromising legal jurisdiction? The obvious recourse was the French school's environmental hypothesis. Not only did it leave the judiciary firmly in control of the legal process but it assured psychiatrists of an influential role and court-appointed financial security. Acceptance of the environmental school also provided for a new class of correctional personnel whose focus would be one of rehabilitation.[12]

.[12] That a new paradigm had been accepted can be seen in several developments in France in the late 1880s. According to Kuhn, the reception of a single paradigm is often a sign of scientific "arrival" for a pre-scientific field. The Criminological Archives, a journal devoted to research and opinion on crime was edited by Lacassange and Gabriel Tarde and together with the formation of specialized societies the French Academy of Moral and Political Science, a paradigm was truly in the final stages of development and acceptance. Finally, the claim of a specialized place in the curriculum conclusively proclaimed criminology to be a science. The 1894 Congress of the International Union of Penal Law called

Explanations for the paradigmatic shift in criminology can certainly encompass the internalists' and externalists' arguments. According to some scholars, a well-documented, theoretically sophisticated sociological explanation was likely to prevail given the inadequacy of Lombroso's methodology and numerous critiques of his work that were well circulated in the academic-scientific community (Nye, 1976). Also, popular notions that crime, alcoholism, and vice were related to such environmental factors as poverty and slum life transcended those which linked deviant behavior to bizarre anatomical peculiarities. Victor Hugo's Jean Valjean, after all, manifested no median occipital fossa; his road to crime was all too clearly one of environmental circumstances.

But there was at the same time a very strong externalist argument featuring the class interests and occupational security of judge and psychiatrist. Environmental theories, with their odd mixture of responsibility and constraint, choice and determinism, allowed court professionals substantial financial and status rewards (Nye, 1976). In looking at this shift in criminology, one is reminded of Michael Mulkay's characterization of science and the sociology of knowledge;

> . . . knowledge is formulated in response to the interests and economic concerns of various social groups; and is constrained by the ideological assumptions current within particular modes of production (Mulkay, 1979:5).

The "various groups" mentioned in the above example were quite clearly constrained by professional (and economic) concerns. Although few would assert that only self-serving financial rewards motivated the French school's acceptance of environmental determinism, the remarkable congruence of opinion among such disparate groups as lawyers, educators, psychiatrists, judges, and correctional workers bears careful analysis. It is the

---

for the practice of law to apply sociology's principles. This conference was held two years after the International Congress of Criminal Anthropology which celebrated the triumphant ascendance of the French School (Nye, 1976:254).

effect of role, professional ethic, and temporal considerations on scientific theory that makes this shift in social science such a fertile area for analysis.

CONCLUSION

Several years ago sociology received one of its greatest intellectual challenges with the publication of Edward O. Wilson's *Sociobiology*. Geneticists, biologists, and others in the natural sciences have been quick to seize upon the relevance of Wilson's "modern synthesis" for their disciplines; sociologists have been conspicuous by their absence. Yet Wilson's work speaks directly to core concepts in sociology; indeed the chapter headings read like a table of contents of any of the ubiquitous "Intro" texts around: roles and caste, parental care, development and modification of social behavior. Sociology's inattention cannot be attributed to any mistake about the scope of Wilson's claim. Consider the following passage concerning the modern synthesis:

> . . . in which each phenomenon is weighted for its adaptive significance and then related to the basic principles of population genetics. It may not be too much to say that sociology and the other social sciences . . . are the last branches of biology waiting to be included in the Modern Synthesis. One of the functions of sociobiology, then, is to reformulate the foundations of the social sciences in a way that draws these subjects in the Modern Synthesis (Wilson, 1975:4).

How are students in sociology to approach this work? How will they respond to Wilson's explantion for stratification which attributes hierarchical position to genetic predisposition? And further, how can a sociological study of the "modern synthesis" affirm the goals of general education?

As students approach what Wilson has written and how he has presented his case, the importance of paradigm as perspective becomes evident. The author's use of fitness, adaptation, and survival is quite similar to the functionalist approach in sociology; for both, individual members of the species are selected because of their inclusive fitness. Available talent is scarce, and because functionally impor-

tant positions must be filled, stratification evolves to ensure that important positions, be they soldier ant or physician, will be filled by the most talented members of the species. But what is the *evidence* of adaptation . . . survival? If one of the goals of general education is to develop a "critical mind in the sense of the capacity to test and challenge . . . previously unexamined assumptions" (Kenniston and Gerzon in Carnegie Commission, 1977:155) shouldn't students look for adaptation as a process, one in which particular characteristics spell survival at one time and perhaps not at another? After all, adaptive characteristics vary not only from human to non human species, but from one human society to another, and even within the same society over time. Wilson's exclusive focus on gene pools as the basic unit in evolution all but dismisses the extent of sociocultural variation and the survival of certain selected forms of human behavior. As Blute writes:

> The relative frequency of alternative sociocultural beliefs wax and wane with time depending upon their *own* adaptiveness to a *sociocultural* milieu and these fluctuations are accompanied by appearances of novelty and events of extinction (1976:730).

Are these beliefs (and practices) *adaptive* because they have survived; did they aid their adherents to survive? Is there perhaps a level of shared meaning that Wilson ignores in his attempt to synthesize human and non human behavior forms? Might this level distinguish human from non human societies in a discussion of the *process* of survival or stratification? Wilson presents the human species to be one of many, showing the reader the continuity of behavioral forms across species. Students will no doubt find his documentation impressive and his thesis compelling, but they also should be led to question why the author limited his investigation to genetic endowment when there exists so much human variation at the sociocultural level.

Another theme to stress in presenting sociobiology to undergraduates is that the creation and acceptance of scientific ideas occurs within describable social and cultural contexts, namely scientific and aca-

demic institutions, and that these contexts are open to sociological study. Sociobiology has been all but ignored by sociologists perhaps because its tenets appear to raise anew the arguments of social Darwinism and Herbert Spencer. If this "modern synthesis" is summarily dismissed simply because of its social implications, i.e., that it tends to confer legitimacy to the status quo, then this dismissal will serve to demonstrate that sometimes the rejection of scientific ideas may have little to do with the internal consistency of a new scientific argument. One wonders whether social scientists share some commitment to a vision of "social reality" that precludes their serious consideration of biological explanations for social behavior.

On the question of internal consistency, Wilson's work also can serve to explore the issue of argument by analogy; because one sees X behavior in ants and birds, and if the same actions are observed in human conduct, does this signify that the behavior has a common genetic base among all living species? How are students to respond to argument by analogy? One can see in the examples presented in this paper the saliency of the metaphor of selection that Darwin found so useful in interpreting chance variation and the preservation of selected forms. Similarly, when French criminologists invoked the memory of Pasteur in alluding to the "fermenting broth" as the medium (one might even say *culture*) in which criminal traits take root and "sprout," they were availing themselves of an already accepted scientific explanation borrowed from another discipline. But are all such "borrowings" as well received; what factors condition the reception of a paired association with another's discipline?

MacLeod (1977) suggests that one place to find an explanation may be the "social function" of these scientific ideas; in both cases cited above, the scientific ideas helped to buttress either political ideology or occupational security. Natural selection as an evolutionary theory did not appear out of a vacuum. Given the themes of competition and struggle so central to Victorian life, one really isn't suprised that *On the Origin of Species* found a

ready audience. And when one thinks of the unique role structure of the French courtroom, it is not hard to account for the eventual triumph of the environmentalist position in criminology given the occupational concerns of judges, psychiatrists, and academic sociologists. To say all of this is not to imply some sort of cultural determinism, where scientific discoveries follow, or are dictated by, social thought. Rather, it is to alert students to the academic forum in which certain scientific problems are posed and others are ignored, to the political context in which some research avenues are opened up for possible exploration and others are closed off as too volatile or "sensitive." One wonders if the growing popularity of sociobiology and renewed interests in biological theories in criminality are responsive to perceived public disaffection with the Great Society social action programs and the failure of rehabilitative measures. Genetics and "kinds of people" theories have been around for a long time; is their new-found respectability attributable to methodological sophistication or to their compatibility with emerging political themes of the new decade?

These are not easy questions to pose in class, and the instructor who is able to extend the scope of class discussion to include a consideration of scientific knowledge as problematic in itself is skillful indeed. Perhaps one of the reasons why this and other goals of general education are so hard to realize is that they demand of instructors considerable self examination with regard to their own intellectual leanings and pedagogical devices. How often does one present material in such a way that students will question the conditions under which ideas and concepts evolve according to some autonomous "internal dynamic" and under what other conditions are these same ideas adapted to temporal and spatial constraints? When students begin to recognize the influence of time and space, of ideology and professional ethic on the way they receive ideas, sociology will have taken an important step in reaffirming its role in general education: the fostering of intellectual curiosity and the ability to

challenge uncritical loyalties and provincialism.

REFERENCES

Astin, Alexander
1977 Four Critical Years. San Francisco: Jossey-Bass.

Bernal, J. D.
1954 Science in History. London: C. A. Watts and Co. Ltd.

Blute, Marion
1976 Review of *Sociobiology* by Edward O. Wilson. Contemporary Sociology 5(6):727–731.

Carnegie Foundation for the Advancement of Teaching
1977 Missions of the College Curriculum. San Francisco: Jossey-Bass.

Darwin, Charles
1859 On the Origin of Species. London: Murray.

Gale, Barry G.
1972 "Darwin and the concept of struggle for existence: A study in the extrascientific origins of scientific ideas." ISIS 63:321–344.

Gould, Stephen J.
1977a Ever Since Darwin. New York: W. W. Norton and Company.
1977b Ontogeny and Phylogeny. Cambridge: The Harvard University Press.

Greene, Joseph C.
1959 The Death of Adam. Ames: The Iowa State University Press.

Haberer, Joseph
1969 Politics and The Community of Science. New York: Van Nostrand Reinhold Company.

Harvard University
1978 Report on the Core Curriculum. Unpublished memorandum, February 15; Office of the Dean.

Johnson, Ron
1976 "Contextual knowledge: A model for the overthrow of the internal/external dichotomy in science." Australian and New Zealand Journal of Sociology 12:193–203.

Krohn, Roger
1977 "Scientific ideology and scientific process: The natural history of a conceptual shift." Pp. 66–99 in E. Mendelsohn, P. Weingart, and R. Whitely (eds.), The Social Production of Scientific Knowledge. Boston: D. Riedel.

Kuhn, Thomas
1962 The Structure of Scientific Revolutions. Chicago: The University of Chicago Press (enlarged edition 1970).
1977 The Essential Tension. Chicago: The University of Chicago Press.

Lombroso, Cesare
1876 L'Uomo Deliquente. Milan: Hoepli.

MacLeod, Roy
1977 "Changing perspectives in the social history of science." Pp. 149–195 in I. Spiegel-Rosing and D. des. Price (eds.), Science, Technology, and Society: A Cross-Disciplinary Perspective. London: Sage.

Mannheim, Karl
1936 Ideology and Utopia. New York: Harcourt Brace Jovanovich.

Mendelsohn, Everett
1977 "The social construction of scientific knowledge." Pp. 3–26 in E. Mendelsohn, P. Weingart, and R. Whitely (eds.), The Social Production of Scientific Knowledge. Boston: D. Riedel.

Merton, Robert K.
1968 Social Theory and Social Structure. New York: The Free Press (originally published in 1949).
1970 Science, Technology and Society in Seventeenth-Century England. New York: Harper and Row (originally published in 1938).

Merz, J. T.
1923 A History of European Thought in the Nineteenth Century. Edinburgh and London: W. Blackwood and Sons.

Mulkay, Michael
1976 "Norms and ideology in science." Social Science Information 15:637–656.
1979 Science and the Sociology of Knowledge. London: George Allen and Unwin.

Nye, R. A.
1976 "Heredity or milieu; The foundations of modern European criminological theory." ISIS 67(238):335–355.

Overington, Michael
1979 "Some developments in metatheory." The American Sociologist 14:2–12.

Ruse, Michael
1973 The Philosophy of Biology. London: Hutchinson and Company.

Stocking, George W.
1968 Race, Culture, and Evolution. New York: The Free Press.

Wilson, Edward O.
1975 Sociobiology: The New Synthesis. Cambridge: Harvard University Press.

Wolfgang, Marvin E.
1960 "Cesare Lombroso." Pp. 168–227 in H. Mannheim (ed.), Pioneers in Criminology. Chicago: Quadrangle Books, Inc.

Young, R. M.
1969 "Malthus and the evolutionists: The common context of biological and social theory." Past and Present 43:109–145.
1971 "Darwin's metaphor: Does nature select?" The Monist 55:442–503.

COMBINING CONTRACT AND SOCIOLOGICAL RESEARCH: THE NEBRASKA ANNUAL SOCIAL INDICATORS SURVEY*

ALAN BOOTH, LYNN WHITE, DAVID R. JOHNSON, AND JOAN LUTZE

University of Nebraska

Periodic regional surveys are relatively rare. We report on one such survey, which has been underway four years and combines contract research with the sociology department's research, teaching, and service programs. The organization of the project and the results of a recent evaluation are described in detail. The problems and prospects of starting an annual survey also are discussed.

Periodic national surveys such as NORC's General Social Survey, University of Michigan's Election studies, and the decennial Census are commonplace. With the exception of the Detroit Area Study (Schuman, 1977), periodic regional surveys are quite rare. Limited resources, tight budgets, and organization problems have prevented such surveys from developing. Recent experience at the University of Nebraska suggests that periodic regional surveys are now possible.

During the last four years, the Department of Sociology at the University of Nebraska-Lincoln has been engaged in a unique attempt to combine contract research with basic social science research in an annual statewide social survey. As we have a steadily growing number of articles, papers, and theses coming from the survey data and have more demand from potential contracting agencies than we can handle, we feel our experiment has been successful. The purpose of this article is to describe in detail the history of the Nebraska Annual Social Indicators Survey (NASIS), the way it operates, and the results so that other Sociology Departments might explore the feasibility of undertaking what has been for us a most fruitful enterprise. In Nebraska the survey has proven productive for faculty and graduate student research interests, supported an on-going research service apparatus, gained support for the Department and the College, as well as served the needs of contracting public agencies.

HISTORY OF NASIS

Since 1965 the Sociology Department has had a Bureau of Sociological Research in charge of providing faculty and students with research services such as keypunching, coding, programming, interviewing, and terminal access to the central computer. While receiving some support from federal grants and from the College of Arts and Sciences, contract research seemed to be crucial to providing a full complement of services on a sustained basis. After ten years of experience, however, we had to conclude that contract research was not beneficial to the Department's research program. Large projects taxed the staff and facilities and interfered with providing research services to faculty and students. The periodic and largely unpredictable schedule of contract research made it difficult to maintain a staff. Perhaps more important, none of these contract research products led to a single piece of scholarly research, not even a single thesis, in spite of large inputs of faculty time.

One of the factors which led to a disillusionment with contract research was the gross inefficiency and wastefulness of the contract research we were doing. No sooner had we completed a statewide survey for one agency, than another would request a similar survey of the same population—asking about half of the same questions. This suggested to us that perhaps we could manage in one statewide survey each year to produce, efficiently, contract survey data for state agencies, regularize our work schedule, and produce data for basic social science research.

The idea had considerable appeal from the start. The idea of a joint University-State project appealed to the University Administration. It was seen as a major vehicle for providing public service and improving public visibility of the University. The Dean was especially pleased as it is seldom that Arts and Sciences has a chance to provide public service in a direct way. A seed money grant of $6000 was made available, as were additional equipment funds and a commitment to fund part of the cost of some of the Bureau's support staff.

Our first step was to write to all State agencies we thought might be interested and invite them to join an advisory group. Two one-day workshops and numerous meetings were held to acquaint planning and research directors of agencies with the project and to familiarize them with sample surveys, study design, and measurement problems. Considerable effort went into making state and local government agencies aware of the prospects and problems associated with the use of survey data. This advisory group continues to meet and their advice is highly regarded and valuable to the project.

These early efforts have been rewarded. Although not self-supporting in its first three years, the 1980 and all future surveys will earn enough through their contract component to meet costs for the entire survey project, including basic social research. The project is on its way to becoming institutionalized among state agencies in Nebraska: some agencies buy every year, some every other year, and we get unsolicited inquiries each year about survey participation.

STUDY DESIGN

NASIS is an annual survey which alternates between a cross-sectional and a panel component. Each year we interview a sample of approximately 1800 noninstitutionalized adults over the telephone.[1]

On odd-numbered years we interview members of a panel that began in 1977. On even-numbered years a new trend sample is interviewed. Some members of the trend sample become part of the succeeding year's panel to keep it representative. Eighteen-year-old respondents (new adults) and new migrants to the state are added to the panel from each trend survey. Members of the panel lost by migration and refusal are also replaced, being selected at random from members of the trend survey who are of the same age, sex, and rural-urban residence.

Sampling is done by random digit dialing (RRD) and the interviewing by telephone from a central location. For detailed descriptions of the RRD procedure see Klecka and Tuchfarber (1978). In the first year of the survey, one half of the sample was selected by RRD and interviewed by telephone and the other half selected by conventional area probability techniques and interviewed in person. Careful analysis of the representativeness and data quality of these two sampling-interviewing techniques demonstrated fewer statistically significant differences than would occur by chance. These comparisons have proven useful in justifying the use of the less expensive telephone interview to potential contracting agencies.

Each year's interview is divided into three parts: core questions, basic research questions, and contractor's questions. In a typical survey, 25–30 minutes are sold, 8 minutes are core questions, and 7–12 minutes are basic research questions. The core questions consist of basic demographic variables and selected indicators of quality of life. The demographic questions cover age, sex, family status, education, migration, employment, ethnicity, religious affiliation, marital history, income, and residence (community and housing). The quality of life items solicit respondent's satisfaction with community, neighborhood, job, family, and fi-

---

[1] A sample size of 1800 was selected on the basis of input from a wide range of sources. Most potential users indicated a sampling error of three percent at the .05 level of confidence was acceptable. In addition, it provided subsamples of at least 100 for each

planning region in the state. While different agencies use different regions, there is one set that is used more than others. Region is coded into the data as is county which is the basis for most other planning region designs.

nancial prospects. Items dealing with the individual's health, political participation, and personal safety are also asked. Most of these items are worded to conform to national studies such as NORC's General Social Survey, the National Health Survey, Law Enforcement Assistance Administration victimization surveys, National Election surveys, and the like.

The fall is devoted to working with potential contractors and drafting and pretesting items. It is on the basis of the pretest that the interviewing time for each agency is determined; a minute of interviewing time on the 1980 survey sold for $1500. The entire schedule is pretested in early December by the interviewers who are going to be doing the survey. The telephoning is done from February through April from the Bureau offices by experienced, professional interviewers. The data are coded, punched, verified, put on tape, and cleaned during the succeeding three months. Data are available in mid summer so that faculty can work with it over the summer and agency personnel can begin work on reports due for the legislative session beginning in January.

For their $1500/minute contractors get frequency distributions on their items and on some background variables plus crosstabulations among these. Additional analysis requests are met on an at-cost-basis. Agencies wishing complex analysis are urged to buy a copy of the entire data set on tape and do their own analysis. Faculty and students also carry out their own analysis. All purchasers of time on the survey may have exclusive use of their data for one year. So far, however, all buyers have waived this right and made their items available to other users. The only restrictions are that users file a statement with the NASIS director before using the data and that permission of the original contracting agency be sought before any of their items are used in a public report. Faculty and students have free access to the data once they are collected, but are under the same obligations of courtesy noted above. The statements filed with the Director ensure that two groups are not working on the same problem.

ORGANIZATION

NASIS is an integral part of the Department of Sociology. Its Director is a full-time faculty member who is elected to a two-year renewable term as both Study Director of NASIS and Director of the Bureau. In return for being Director, this individual gets a one-course-per-semester reduction in teaching load, a summer salary, and a few minutes of time on the survey.

The Director is responsible to the entire faculty of the Department of Sociology which serves as a Governing Board of the Bureau and which makes all broad policy decisions. More immediate guidance is provided by an Executive Committee consisting of the Director, the Chair of the Department, the previous Bureau Director, and two to three faculty members who volunteer for the committee. Of the latter, those who have a sustained interest in the project participate every year while others participate less often. Members of the Executive Committee make decisions on the length of the interview (usually 45 minutes), the portion of the overall interview that is to be sold, and provide general guidance to the Director. In return for their service, members of the Committee determine the basic research material that forms a part of the survey and have first access to these data.

CONSEQUENCES OF NASIS

To assess the impact that NASIS has had, we consider its effect on faculty and student research productivity, its use for instructional purposes, and its effects on public policy as measured by the use reported by buyers and selected nonbuyers.

Sociology faculty and students were surveyed about research based on NASIS data in a two-year period starting with the availability of data from the first survey. Twenty-seven separate research projects were undertaken by seven different faculty and six graduate students. Eight papers have been published or are in press, six papers were read at meetings, three theses were completed and three are underway, and there are seven papers still

in various stages of preparation. Two people from the Political Science department at Nebraska have published three articles using NASIS data and have several other projects underway. Three sociology departments in other institutions in Nebraska have purchased NASIS data sets on a regular basis, but we are unaware of the extent to which they have been utilized by these organizations. Overall, it has been a rich source of data for a significant number of faculty. The panel data just now available should accelerate the use of the data for scholarly research. One political science professor received two grants to buy time on the survey. Five sociologists have obtained grants to undertake special analyses of NASIS data.

The use of NASIS in the instructional program is varied. In some courses data from the survey are reported in lectures. In other cases, data from the survey are used to illustrate certain methodological techniques. In the two-year-period NASIS was used in ten different courses which enrolled more than 2500 students. Data reported to introductory classes account for eighty percent of student contact with NASIS. Other classes using NASIS data are: introductory and advanced methods courses; women in contemporary society; delinquency and crime; marriage and the family; population trends and problems; classes in urban sociology; social institutions, and policy and program evaluation. Extensive use of the data in the larger lower-level courses means that most students who take sociology courses come into contact with NASIS at one time or another.

The effect of NASIS on public policy is much more difficult to assess. Our goal has been to provide accurate information so that more informed decisions can be made, not to advance a particular point of view. The project affects two sets of people: the public and policy makers. The public's contact with the project is primarily through the NASIS reports published each fall. Each year a dozen or more of these reports are written and distributed to the news media about survey findings. They cover topics of general interest such as: quality of life in Nebraska, Nebraskans' views of their environment, fertility, marriage, and divorce in Nebraska, ethnic groups, political participation, people's views of their State Legislature, residential mobility, criminal victimization, energy conservation, and numerous other topics. Typically, these reports get considerable media coverage. In addition to the report itself, a news release is prepared and distributed to media all over the state. Articles appear on every report in the major newspapers in the state and probably in many local papers. At least five radio stations and the local T.V. station carry, on a regular basis, an item on each report and usually a clip from a conversation with its author. A taped interview prepared by the University Information Office is sent around to a number of radio stations in the state. Some attention is given regularly to NASIS on a weekly University news program. The extent to which these efforts inform people about public issues is something about which we can only speculate.

Some reports have captured the attention of policy makers. A report showing citizens had little information about their state legislature generated considerable discussion among legislators and, as a result, they made attempts to improve their "image." A report on citizen evaluation of the fairness of property, sales, and income taxes has been cited by the Governor. On the other hand, reports about marriage and divorce, religious participation, and rural-urban differences have not, so far as we know, influenced decisions of the voting public or policy makers. We have heard, moreover, of instances where ministers incorporate these materials into sermons, and our reputation as a source of information about the state has grown to the point where we regularly receive requests for information from news media, researchers, and planners for state and private agencies. So far we have received no negative press coverage; perhaps, because we are a source of news that is dependable and cooperative.

Information on the direct use of NASIS information by policy makers and pro-

gram administrators in public agencies was obtained from a phone survey of representatives of nine agencies who have bought time on the survey.

Agencies which have purchased time represent the areas of health planning, welfare, crime, environment, alcoholism, recreation, aging, extension, and economic development. Representatives from these agencies were questioned about reports they had prepared using NASIS data. The nine agencies had compiled 28 reports and papers in the period since NASIS began (which is slightly more than two years before the survey of policy and program administrators was undertaken). Reports were prepared for other administrators within the agency and in affiliated groups and for general consumption, such as annual and special reports intended for distribution to libraries; use as teaching aids in schools; for citizens who request information; and constituent groups. Three agencies used NASIS data as the basis for statewide plans. Several agencies used the data to prepare special reports to the Governor and to members of the Legislature. A few agencies used the data to prepare reports required by the Federal government. One agency used the data as the basis of a technical report to be published in a professional journal.

When asked: "Has your agency used NASIS to try and change or create new policies or program?" five of the nine responded affirmatively (crime, aging, health, economic development, welfare), and another indicated it would do so in the near future. In most instances NASIS data was used as background information to justify new policies and was part of a larger package of evidence rather than the sole source of information. In one instance, however, the data were used as the basis for allocating funds within the state—in this case it was the basis for implementing rather than changing policy.

NASIS has had the effect of actually increasing the use of research data in the operation of one-third of the agencies. In response to a number of questions about the quality of NASIS data, users deemed it a source of information that is reliable, easy to work with, and worth the money.

Agency representatives also were asked about their sources of data before 1977 for which they now use NASIS. Many reported conducting their own mail and phone surveys, while others indicated they had drawn on data supplied by federal agencies. Overall, then, it appears that NASIS is used extensively and that the data has some effect on public policy. It is seen as an improvement over sources used previously and users appear to be quite confident in and pleased with the project.

STARTING AN ANNUAL SURVEY: PROBLEMS AND PROSPECTS

The Department of Sociology at Nebraska was in a good, but not unique, position to start a contract research/social science survey. Our past experience with contract research had built up a network of state agencies who had already worked with us, an experienced team of clerical and interviewing staff, and established procedures for running both social surveys and contract research through the University and state systems.

From our experience in Nebraska and from talking over the idea with people in three other states who have sought our help in starting a similar survey, the easiest step in beginning an annual survey is finding interested state agencies. Some agencies already run surveys, others are in need of survey data in order to plan or implement programs. Our format frees them even from drafting contracts and supervising data collection. It is an extremely cost-efficient method of gathering data. We estimate that by participating in an omnibus survey, agencies cut their survey cost by one half over what it would cost them to implement a comparable survey design. During the past three years we have had more agencies desirous of participating in the survey than we could accommodate without giving up some of the social research aims of the study. We accommodate agencies on a first come-first serve basis to avoid oversubscription.

Looking back over our experience and talking with people in other states suggests that our decisions to maintain full control of the survey within the depart-

ment is crucial, as is building the survey from the bottom up. We did not try to launch the project by getting support from the Governor and other high state officials or even high University officials. Our approach to agencies has been through people in charge of research and planning and not through agency directors. We have heard of two instances where approaches through the top administration has lead to attempts by high government officials to censor questions they felt were "too sensitive." By giving state agencies no direct control we have not encountered attempts to take control of the design of the survey which we understand has happened in other states where such omnibus survey attempts have been tried.

The real problems in setting up an annual survey rest in securing professional interviewers and a good clerical staff. Random digit dialing, while inexpensively producing a good random sample, is not a satisfactory experience for most interviewers: too much paper shuffling and not enough interviewing. Further, a 45-minute interview is difficult to sustain. Fewer than one percent of our respondents quit before the end of the schedule, but it does take skill to maintain the respondents' interest. Our history of contract research makes it possible to maintain a team of 20 professional interviewers who work for us for these three months each year. During the rest of the year they work for NORC, CPS, and local survey organizations.

The other major difficulty is getting through the red tape. Most university personnel systems are not well equipped to deal with interviewers, temporary but relatively well-paid staff hired outside of any system-wide merit system. It also may take some negotiations to arrange special telephone rates. Even when a university has a WATS line, it is reasonable to try to negotiate lower weekend or evening rates based on very extensive usage.

Even for research organizations with previous experience with contract research, it probably would require a one-year lag between starting a project such as this and beginning the first interview. Even in the fourth year of NASIS, when the buyers are largely familiar and the procedures have become institutionalized,

work on the survey begins six months prior to the first interview.

THE NASIS EXPERIENCE: AN OVERVIEW

Beginning the NASIS project is a decision that we have not regretted. It has been good for the Department of Sociology, faculty as well as students, for the College of Arts and Sciences and the University, and for the state agencies involved.

From our point of view one of the major benefits is the significant amount of scholarly research that has flowed from the survey. With the availability of the panel and cross-sectional trend data, we expect this to increase substantially. In addition, while we have not conceived of the survey primarily as a means to train graduate students, it has been instrumental in this regard. All of our students are given an opportunity to be involved in the survey in either programming or analysis capacities. In order to accommodate as much interest as possible, all students interested in working on the analysis phase (doing analysis for buyers) can sign up for a 4/hr/week overload to their regular funding to do programming. This gives them experience in working with data and, as much as possible, in research decision-making. Out-going graduate students who have been involved with the project are making excellent use of their experience in getting university-affiliated or government jobs requiring expertise in data management, consulting, and data analysis. Graduate students also are urged to add to their vita and skills by writing a NASIS report for us and to explore the possibility of turning it into a journal article. Graduate students wishing to add items to the survey of thesis or dissertation research are allowed to work off the cost of their items by helping with coding, programming, and analysis. Thus, while not designed primarily as a training vehicle, the survey has been instrumental in teaching survey research methods and large data base analysis to our students.[2] In addition to these direct

---

[2] In any heterogeneous department there are, of course, individuals who do not want to be involved with survey research. The fact that students of all

benefits of the survey, there is a latent consequence of having survey apparatus already in place. Each additional study proposed by faculty or students is both easier and cheaper to accommodate.

The costs necessary to achieve these results are relatively small. The actual costs of the survey are paid for by the contract component. The major cost to the Department is the forfeiture of two courses per year by the Study Director. The University Administration pays for leasing three computer terminals, maintaining a keypunch and verifyer, and the

---

persuasions are not coerced into working on the project has been essential in maintaining support for the project from the entire Department.

salaries of a data entry leader and a secretary.

Overall, then, the NASIS experience has been a fruitful one for the Department and the public agencies; it has proven to be regarded highly as a public service to the state and as a means of improving the public image of the College and University.

REFERENCES

Klecka, W. and A. Tuchfarber
  1978 "Random digit dialing: A comparison to personal surveys." Public Opinion Quarterly 42:105–114.
Schuman, H.
  1977 "The Detroit Area Study after twenty-five years." The American Sociologist 12:130–137.

COMMUNISM IN NORTH AMERICAN SOCIOLOGY: A STUDY OF THE RELATIONSHIP BETWEEN POLITICAL COMMITMENT AND SOCIAL THEORY*

RICHARD SWEDBERG

Harvard University

The suggestion is made here that sociology is not "value-free" because this perspective cannot account for the way sociological works are actually generated. To illustrate this point, the author looks at the way communism has been treated in mainstream North American sociology. When one examines the various sociological works on communism one notices that the different scholars—Selznick (1952), Bell (1952), Shils (1954a), Stouffer (1955), Howe and Coser (1957), Lipset (1960), and Glazer (1961)—decided to pursue certain issues rather than others because of their initial opinions of communism. Since most of these studies are strongly anti-communistic a one-sided picture of communism has resulted. Finally, a plea is made for a more sophisticated analysis of the ways in which a scholar is influenced by the society he/she lives in, so that one might more fully understand the complex relationship between political commitment and social theory.

People are too emotional about communism or rather about their own communist parties, to think about a subject that one day will be a subject for sociologists.

Doris Lessing, *The Golden Notebook* (1962)

---

* Address all communications to: Richard Swedberg, Widener Library, Harvard University, Cambridge MA 02138.

The way a social scientist's political commitment influences his or her scholarly work is one of the most controversial questions in social science. The two main traditions that dominate the debate are the Hegelian-Marxian one, which stresses that social science plays a political role in the sense that it is part of the unfolding whole of society, and the Kantian-Weberian tradition, which emphasizes that social science, if properly handled, can be kept outside the political arena.

Hegel and Marx saw society from the perspective of totality and conceived their own theories as transcending academic divisions as well as the distinction between theory and practice. Kant and Weber, on the other hand, propounded the view that politics and intellectual analysis are two distinctly different matters, and both men operated within the academic divisions of labor of their respective periods. Most contemporary contributions argue around these two positions (see, for example, Gouldner, 1962, 1968; Becker, 1967; Friedrichs, 1968, 1970; Nicolaus, 1969; Denzin, 1970; Berger, 1971; MacRae, 1971).

My purpose in this article is not to try to arbitrate this debate. I would, however, propose the following point: What is most important in social science in general and perhaps also in this question in particular is the explanatory power of a hypothesis. If one grants this idea, the following question arises: Which of the two traditions—the Hegelian-Marxian one or the Kantian-Weberian one—can best explain how a certain type of analysis is *generated*. If one supports the Hegelian-Marxian tradition it is necessary to show that social theory is generated mainly from concerns *outside* the sphere of the sociologist's role as sociologist, such as from his/her relationship to major social institutions or from his/her role in the class struggle. Among the scholars who have tried to show a close relationship between sociological theory and society one can mention Goldmann (1950), Lukács (1954), and Schwendinger and Schwendinger (1974). Those working in the Kantian-Weberian tradition should be able to show that social theory is generated from concerns that fall mainly within the social science community, for instance, from communications with other scholars (network analysis is an example) or from the "sociological tradition" in general. Some well-known studies focusing on the profession of sociology have been authored by Sorokin (1966), Clark (1973), and Mullins (1973).[1]

My position is that the Hegelian-Marxian tradition has the most explanatory power. I shall try to show this by looking at how communism has been treated in mainstream American sociology.[2, 3] In each of the major works that is reviewed I shall try to discover exactly what structures or generates the direction of the research. I hope to show that the essential factor in the various researches

---

decides what is "science," and he considers that the existing "paradigm" suggests what is worth investigating and which solutions are acceptable. (I thank David Samuel Kruse of the Kennedy School of Government, Harvard University, for this and other helpful observations.)

[2] The works on communism by North American political scientists, anthropologists, and social psychologists are not discussed in this article, but those works which from a general point of view can be labelled "sociological" deserve brief mention here. The most famous work by an anthropologist is Margaret Mead's *Soviet Attitudes Toward Authority* (1951). Among political scientists, three scholars stand out: Harold Lasswell, Gabriel Almond, and Barrington Moore. Harold Lasswell has essentially tried to apply the techniques of content analysis to communism. See, for instance, his articles in Lasswell et al., *Language of Politics* (1949). Lasswell and Blumstock's *World Revolutionary Propaganda: A Chicago Study* (1939) can be said to be the first sociological study of North American communism. Gabriel Almond's influential *The Appeals of Communism* (1954) is far more sophisticated in research techniques than most works in mainstream sociology, but suffers from a similarly narrow perspective. Barrington Moore's *Soviet Politics* (1950) is an attempt to see how the original ideas of the Bolshevik Party had to be adjusted to the realities of exercising power. *Terror and Progress* (1954), by the same author, is a weaker work which applies a "social systems" approach to the Soviet Union. Among the large number of recent "sociological" works in political science on communism, see especially Donald L. M. Blackmer and Sidney Tarrow, eds., *Communism in Italy and France* (1975). Among the social psychologists, see for instance Hadley Cantril's study of French and Italian communists in *The Politics of Despair* (1958).

[3] My reason for not covering works that directly treat socialist states is because studies of socialist systems raise questions that cannot be adequately dealt with in the limited space here. (I thank Theda Skocpol, Harvard University, for this observation.) In passing we can note that the early works in mainstream sociology on the Soviet Union (Inkeles, 1950a, 1968; Inkeles and Bauer, 1959; Bauer et al., 1956) focus on "vulnerabilities" in Soviet society; lesser known works on the Soviet system are more eclectic, such as Davis (1930, 1933), Sorokin (1944) and Timasheff (1946). The two major works on China by North American sociologists (Yang, 1965; Vogel, 1969) are not as explicitly anti-communist as most works on the Soviet Union.

---

[1] In the philosophy of science Kuhn (1962) could be said to reside in the Kantian-Weberian tradition since he focuses on how the scientific community

is the way the scholar *initially perceives communism,* a factor that falls outside the narrow tradition of social science itself. The scholars who are reviewed in this paper come from different intellectual traditions within the sociology profession. But though this helps to explain some twists and turns of their analyses, it does not, in my opinion, account for the general drift or structuration of their research. To find this information one has to look at what the researcher tries to accomplish with his/her analysis, and this, I propose, is intimately connected with how he or she perceives communism. I also have tried to limit the use of information about the various political activities of the scholars in question. While this type of data is extremely important, it should not replace an analysis of the scholarly works themselves.

SELZNICK: COMMUNISM AS ORGANIZATIONAL MANIPULATION

The first studies of communism by North American sociologists appeared in the early 1950s. If one focuses on studies of communism in general—as opposed to works exclusively on Soviet communism (Inkeles, 1950a)—the year the first study appeared is 1952, and the work is Philip Selznick's *The Organizational Weapon: A Study of Bolshevik Strategy and Tactics.*

Selznick's (1952:16) study was explicitly intended as "an advanced training-manual for anti-communistic forces." Selznick (1952:315) argues:

> The more we understand about the nature of communism, the more readily can we avoid (1) the failure to recognize its true aims and subversive methods and (2) those excessive reactions which threaten themselves to undermine the foundations of democratic society. Increased knowledge helps us to think concretely, to specify the problem in situational terms, to direct counter-measures to those areas which are relevant and useful, and to avoid unsought consequences for the integrity of our institutions.

The goal of Selznick's analysis is thus to help crush communism, and this structures his whole analysis. Before examining this fact in more detail, however, I shall summarize Selznick's work.

Selznick's argument, built on the author's expertise in organizational sociology, is the following: democratic societies function on the basis of a certain apathy among the electorate. People participate in the political functioning of the community through voluntary organizations in a half-hearted way. The communists, on the other hand, have a long tradition of manipulating the masses through organizations, and the "combat party" is an expert in organizational tricks. Communists infiltrate organizations and then, especially if the members are hostile to the party, try to manipulate them. The strategy of the United Front, for instance, was especially indicative of this type of deceptive behavior.

For Selznick (1952:33) there is little difference between the behavior of the Bolshevik Party under Lenin and under Stalin; Stalinism is merely "mature Bolshevism." He also argues that the Communist Party of the United States (CPUSA) had no mass following, but was nevertheless involved in undermining the country. CPUSA, according to Selznick (1952:72, 176–177), had in 1952 the power to seriously damage an "essential industry" and to split the CIO. In addition, every party member was a potential spy.

Selznick's study reads today more like a Cold War tract than a scholarly study. Selznick, however, makes some useful sociological observations as well. For instance, he discusses (1952:25) how a CP mobilizes its members through what he calls "insulation" and "absorption." The former refers to the party's attempt to isolate its members from capitalist society, and the latter to the heavy commitment that a CP often demands from its members. Another good sociological observation that Selznick makes is that the Bolshevik Party during its formative years had to exaggerate its differences with the reformistic Social Democratic parties in order to establish a distinct and separate political identity. Also interesting is Selznick's (1952:64) contention that a CP always has to seek a balance between alienating itself too much from capitalist society ("leftist sectarianism") and not separating itself enough ("right opportunism").

On the whole, however, Selznick's attempt to analyze communism from the perspective of organizational sociology is not very fruitful. By this we do not mean that Selznick's strong anti-communism *per se* invalidates his study. To label Selznick "anti-communist" and then, on this ground, to dismiss his study is simplistic. A different and more useful way to proceed, as I have suggested in the introduction, is to look at how Selznick conceptually perceives communism and what insights he can possibly generate from this perspective. For Selznick, communism is basically an alien phenomenon, something that does not belong in a democratic society. Selznick thus perceives of communism as essentially separate from society and as something that is not produced by its main institutions and structures. His study of communism consequently precludes a solid analysis of society's structures from the very beginning. Having cut himself off from a historical-structural approach to communism, Selznick has to resort to weaker theories to explain his findings. This trend manifests itself strongly in his tendency to overplay his initial insight—namely that there is a sophisticated use of organizational knowledge in the Leninist tradition—and to reduce the extremely complex communist phenomenon to such one-dimensional categories as "manipulation" and "propaganda." In brief, Selznick's perception of communism as an "alien object" prevents him from making a penetrating analysis of it.

SHILS: COMMUNISM AS LEFT-WING AUTHORITARIANISM

Two years after Selznick's work, in 1954, another study of communism by a North American sociologist appeared: Edward Shils's article "Authoritarianism: 'Right' and 'Left'," in *Studies in the Scope and Method of "The Authoritarian Personality,"* edited by Richard Christie and Marie Jahoda (Shils, 1954a). What generated Shils's study was an attempt to find similarities between fascism and communism. Shils's article has had a great ideological impact and can be said to constitute, next to S. M. Lipset's *Political*

Man (1960), the most important statement by a North American sociologist on the nature of communism.

Shils's argument consists of two parts: one very general and theoretical section, and a second section in which Shils, with the help of data from *The Authoritarian Personality*, tries to show that left- and right-wingers have similar authoritarian personality structures.

Shils's theoretical argument is quite interesting, for it constitutes an attempt to understand communism from the viewpoint of pluralism, that is, from the notion that extremism destroys the healthy balance of different groups in capitalist society.[4] The extreme right and the extreme left, Shils argues, have more in common than the moderate right and the extreme right, or the moderate left and the extreme left. To see the right and the left as polar opposites might have had its justification in the 19th century, Shils argues, but not any longer—the "Right-Left continuum" is "obsolete." To illustrate Shils's thought graphically:

19th century		20th century	
Left	Right	Extremists (right and left)	Pluralists (right and left)
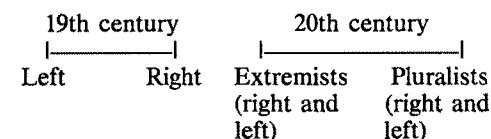
Exactly what the extreme right (fascists as opposed to conservatives) and the extreme left (communists as opposed to socialists) have in common is not quite clear from Shils's article. The most complete answer he gives is the following (1954a:27–28):

> Fascism and Bolshevism, only a few decades ago thought of as worlds apart, have now been increasingly recognized as sharing very many important features. Their common hostility towards civil liberties, political democracy, their common antipathy for parliamentary institutions, individualism, private enterprise, their image of the political world as a struggle between morally irreconcilable forces, their belief that all their opponents are secretly leagued against them and their own predeliction for secrecy, their conviction that all forms of power are in a

---

[4] Shils (1954b:225–238) gives a fuller indication of his ideas on pluralism in the closing chapters of *The Torment of Secrecy*.

hostile world concentrated in a few hands and their own aspirations for concentrated and total power—all of these showed that the two extremes had much in common.

Without dwelling too much on the parallels that Shils advances, it is clear immediately that on at least one point he is wrong ("antipathy for . . . private enterprise") and on several others he focuses on social-psychological variables ("predilection for secrecy," "conviction that all forms of power are in a hostile world concentrated in a few hands," etc.) rather than on social structure.

Shils returns to his idea that there are social-psychological parallels between fascists and communists in the second part of his article, in which he tries to show that the data from *The Authoritarian Personality* support this idea. There is no point in going into the details of Shils's argument. It must, however, be pointed out that his hypotheses inspired a number of research projects trying to prove that communists and fascists have similar personalities. In summing up the research conducted during the ten years immediately after Shils's study, Roger Brown (1965:542) drew the following conclusion in *Social Psychology:*

> . . . it has not been demonstrated that fascists and communists resemble one another in authoritarianism or in any other dimension of ideology. No one thus far has shown that there is an authoritarianism of the left. Still the impression persists that such a type exists and that some communists belong to it.

At another point, Brown (1965:528–529) says:

> All of these communist samples [used in the research on authoritarianism of the left] have been absurdly small and probably unrepresentative of total membership. Still the consistently low scores, always on the equalitarian side of neutrality and apparently near the bottom of the range for all groups tested, strongly indicate that Communists *in democratic countries* do not produce high scores on the authoritarianism scale.[5]

The most interesting aspect of the debate about "left-wing authoritarianism" is

---

[5] For another view of the same question, see, for instance, Barrie Stacey (1978:127–131) on "Authoritarianism and working-class communists."

not that it has been more or less settled by now but that it took place at all. It illustrates that knowledge is generated and social theory shaped according to large social interests. In Shils's study and those that followed it, the researchers essentially tried to find similarities between communism and fascism—a quite common concern during the Cold War (Adler and Peterson, 1970). If they had proceeded from a historical and/or social structural perspective their task would have been much more difficult; it would have entailed a detailed comparison of, for instance, the diverse social structures and historical backgrounds of countries like Nazi Germany and the Soviet Union. But to focus on the personality structure of communists and fascists provided a much more ambiguous and convenient area. The result was voluminous writings on an alleged "authoritarianism of the left" (or "red fascism" as some called it).

STOUFFER: COMMUNISM AS A THREAT TO LIBERALS

During the 1950s there was a series of studies on McCarthyism and all of the studies, to some extent, dealt with the issue of communism (Stouffer, 1955:274–278; Bell, 1955; Lazarsfeld, 1955; Lipset and Raab, 1970). The most famous of these works is Samuel Stouffer's *Communism, Conformity and Civil Liberties* (1955), which is based on an extensive survey carried out during the summer of 1954. Six thousand people were interviewed and a very sophisticated method was used.

The goal of Stouffer's study was to discover whether McCarthyism threatened civil liberties and if so, what could be done to stop the trend. This purpose informs his whole analysis and accounts for his decision to look into some aspects of communism rather than others.

Using different indicators Stouffer began by measuring tolerance in the United States. He focused on public tolerance of four groups in society: socialists, communists, atheists, and people who had been accused of being communists but who denied the charge. His results showed that the public was very hostile to

all of these groups, especially communists. Stouffer also tried to identify which groups of people were most ready to set aside the civil rights of others. The following groups tended to be intolerant: churchgoers, women, people with little formal education, people in rural areas, and people in the South. On the whole, "community leaders" tended to be more tolerant than others.

Stouffer's (1955:236) next concern was to decide how to improve the situation, how to "disentangle," as he put it, "the evils of Communism and the dangerous disregard of civil rights." Stouffer tried to show that the degree of tolerance was related to the perception of the threat of communism. He, however, did not believe in a simple one-to-one relationship between the two variables; respect for civil rights, consequently, could not be restored by simply reducing the threat that communism posed. Stouffer's plan of action consisted, in fact, of both short-term factors, such as media campaigns, and long-term factors, such as education. Industrial society, Stouffer argued, would ultimately eliminate intolerance with its own internal dynamic.

Stouffer's study contains a multitude of data, the most famous of which is that less than one percent of the people in the U.S. worried about communism in 1954. We shall not focus, however, on this aspect of Stouffer's study but rather on the relationship between the study's political and cognitive dimensions. Like the researches of Selznick and Shils, Stouffer's work is anti-communistic and occasionally lapses into the liberal rhetoric of the 1950s. Indeed, the closer one looks at the study the more it becomes apparent that Stouffer's ideas directly parallel the concerns of many liberals during the Cold War: how to suppress communists without giving the right-wing forces a chance to suppress liberals at the same time. Stouffer (1955:59, 221–222) makes very clear that in "our capitalist democracy" with its "free marketplace of ideas," the "threat of Communists" is no joke, and that it is all right to suppress communists, as the limits for civil rights have to be drawn somewhere. The whole study, in short, is an attempt to find out whether the liberal fear of McCarthyism is justified and, if so, what can be done to stop it.

Survey research, which can be adjusted to any interests, was a very congenial research tool for this concern. Nevertheless, a complete focus on people's attitudes toward communism does not do justice to the topic, which demands a far more multi-dimensional approach.

The close relationship between what we can call the "political strategy" of the liberals and the "research strategy" of Stouffer's study which we have posited is confirmed in Glazer and Lipset's critique of the study. Glazer and Lipset make quite an extensive critique of *Communism, Conformity and Civil Liberties* in an article entitled, "The Polls on Communism and Conformity" (1955). They find various weak points in Stouffer's study, such as its disregard for the role of political machinery. Their main criticism, however, is that Stouffer's close connection of intolerance and perception of the communist threat is deceptive from a certain point of view. More precisely, it ignores the group of people who are tolerant and, at the same time, correctly perceive the communist threat to be very great. One can, the authors (Glazer and Lipset, 1955:152) stress, "[be] sincerely worried about communism and think that strong measures are necessary to deal with it." Glazer and Lipset, we assume, belong to this group.

BELL: COMMUNISM AS A RELIGION

Very little attention was paid to the American Communist Party in mainstream sociology until the end of the 1950s and the early 1960s. There is one exception.[6] Daniel Bell, in an essay called "Marxian Socialism in the United States" (1952), briefly discusses the history of CPUSA.[7] This study is informed by the author's contention that communism is an attractive but dangerous illusion.

Bell (1952:3–16) argues that one can conceptualize "Marxian socialism" as ba-

---

[6] Another is *The Negro and the Communist Party* (1951) by sociologist Wilson Record.

[7] Some years before Barrington Moore (1945) presented a less polemical picture of CPUSA.

sically a "secular religion" with "churches" as well as "sects." CPUSA, he argues (1952:122), can best be explained with the help of a "sociology of sects." Using this model, Bell suggests that sects appear in "periods of disorganization," have an inherent tendency to "divide," are preoccupied with rituals of "purity," .and give "cosmic and apocalyptic answers" in troublesome times (1952:122–123). All of these characteristics, in Bell's opinion, fit the experience of CPUSA. The reason for the failure of communism in the United States Bell (1952:13) ascribes to its tendency to favor an "ethic of ultimate ends" over an "ethic of responsibility" (Weber, 1919).

There are some advantages to looking at communism as a sect. Much of CPUSA's organizational history—if by no means all—can best be understood if one sees it as a close-knit organization like a sect. The image of "sect," however, connotes a certain fanaticism and unreasonableness that Bell, for ideological reasons, cherishes. Still, the main problem with Bell's approach is that it is not equipped to deal with the general movement of Western society and the emergence of the communist phenomenon. "Disorganization of society," which is implied in Bell's notion of "sect," is an image which fails to capture the steady development of the working class and its various political tendencies. It suggests the dissolution of the human community and implies that the communist movement exploits this process—a view that might be more congenial to Bell's political beliefs than to a serious study of communism. To conceive of communism as merely a "sect" facilitates Bell's exclusive focus on its properties as a group rather than on the social conditions that have produced it.

HOWE AND COSER: COMMUNISM AND RADICALISM AS DISTINCT MOVEMENTS

A much more substantial contribution to the sociology of the communist movement is to be found in Irving Howe and Lewis Coser's The American Communist Party: A Critical History (1919–1957), which was published in 1957. The generating principle of this study is an attempt to draw a distinction between radicalism and Stalinist communism.

The bulk of The American Communist Party consists of what the authors (1957:507) call a "historical narrative" of the party. This part of the book is characterized by a halfhearted attempt to apply the concept of "sect" to CPUSA, but is really a quick journalistic account of the party's history rather than a stringent sociological analysis. The concept of sect is used to explain a variety of phenomena, such as hostility to deviants in the party, its general voluntarism, and its intense factional struggles. The authors also stress the tendency of the party to become the whole life of the members—a phenomenon Coser (1974) later incorporated into his concept of "greedy institution." The concept of sect by itself, however, is not strong enough to serve as a general theory of the communist movement (as Bell had proposed), and the authors clearly recognize this. For in the last chapter of The American Communist Party, which is entitled "Toward A Theory of Stalinism," Howe and Coser (1957:507) propose to outline a sociological analysis of the communist movement ("a study of [its] structure and function"). It is in this last chapter that one can find these authors' main contribution to the study of communism.

One of the problems discussed in this last chapter is the difficulty involved in explaining the general development of the communist movement. The authors confront a series of Cold War myths. To see communism as the result of a "conspiracy," they argue, ignores the appeal it has had to millions of people. The authors also discard the view that communism can be seen as a "new religion," arguing that this fails to take into account the cynicism that has been an integral part of the communist experience. That the development of communism has been caused by poverty—Truman's "stomach communism"— is rejected as simplistic.

Instead the authors feel that it is the notion of "relative deprivation" that can best explain the rise of the communist movement. They suggest that there was a discrepancy in Western society—much more in Europe than in the U.S.—

between the official ideology of equality and the actual social conditions. Howe and Coser also consider that Stalinism had its greatest appeal in countries that were in desperate situations, such as in Germany after 1923.

The authors' most interesting sociological discussion is devoted to the structure of the communist party. They criticize Selznick for being ahistorical in general and for not having realized that there are "crucial differences" between the Leninist party and the Stalinist party (1957:501). CPUSA, for instance, was quite creative during its "Leninist period" in Howe and Coser's opinion. Its leaders as well as its followers were independent and critical in spirit as opposed to those who later came to dominate the party. During its Stalinist period, CPUSA like other communist parties lost contact with the masses:

> [The CPs were] no longer responsive to the interests, be they truly grasped or totally misconceived, of the native working class. Even as the parties continued to function within the political life of their respective · countries, rarely could anything that happened within these countries determine their fundamental conduct (Howe and Coser, 1957:504–505).[8]

The authors also reject Selznick's idea that the CP was a well-oiled "combat party." The efficiency of the CPs, they argue, generally has been exaggerated (1957:538). "In their bulk, they [the CPs] consisted not of a professional revolutionary elite but of transitory, ill-informed and unreliable members." Perhaps five percent of the party constituted a "relatively stable core" (1957:530).

It is especially the "Stalinist Party" that Howe and Coser try to describe sociologically. Such a party, they argue (1957:538), had "several organizational levels" and correspondingly "several ideologies." The two most important levels were the leaders and the mass of followers. Between these two groups there was an "enormous distance" (1957:530). The

leadership, during the Stalinist period, the authors write, consisted of "apparatus men" that feared independence, were dogmatic Marxists, and lacked contact with the masses. The main preoccupation of these men was the "jockeying of cliques on top of a bureaucratic structure" (1957:535). Strict allegiance to the CCCP was mandatory at the leadership level. Usually the leaders, as opposed to the average member, were told if a political position was "temporary"—in short, they had a certain amount of informal knowledge that never reached the average member.

The members, on the other hand, according to Howe and Coser, did not have to show unqualified allegiance to CCCP. They did not know the "inner Stalinist doctrine" and often did not even know the line of the party on various questions (1957:538). The working class members were thus not attracted to the party because of its Stalinist politics but rather because of its radicalism (1957:550): "To the workers in the Western countries it [Stalinism] appealed in terms of the traditional categories of radicalism." The CPs, the authors argue (1957:526), thus exploited the fact that "the working class members . . . often saw the party simply as a militant organization defending their rights."

The Stalinist party, Howe and Coser (1957:534) stress, also had an organizational structure that prevented opposition from the ranks from growing strong:

> [There was] no possibility for political contact among the basic units of the organization. All relations were vertical. No channels for horizontal contact between the units were permitted.

Howe and Coser's analysis illustrates well how a research project is structured by an author's initial attitude toward his or her object of study. That Howe and Coser's sympathies were with the left is quite obvious—the book is dedicated to Silone and Djilas—and this considerably sensitizes their analysis. Unlike Selznick, they question the Cold War myths about communism, and while for Selznick all CPs are alike, Howe and Closer see a huge difference between a CP in its Leninist

---

[8] On the other hand, as one of the reviewers of this article pointed out, it was during the Stalinist period that CPUSA had the greatest mass support.

and its Stalinist phases. The authors of *The American Communist Party* are also willing to conceive of the average member of the party as something more than a "dupe" or a "naive idealist;" they suggest that a working class member was often attracted to the party because of its aura of radicalism and, once in the party, was quite effectively separated from the "hardcore" leadership.

Several other passages show that the authors' positive attitude to socialism (if not to communism) influenced the kind of questions they asked and made them look for information of little interest to the passionate anti-communist. For instance, throughout the work, Howe and Coser (1957:226) try to figure out why the leadership of CPUSA was so servile: "It is this surrender of will that finally forms the most troublesome aspect of the history of American communism...." Moreover, the question of "American exceptionalism" is treated seriously, and the authors argue that it might have led to "a fundamental and independent revision of American communist strategy" (1957:166). The authors also try to find out exactly why the membership had such difficulty making their ideas heard by the leadership—hardly a concern that Selznick undertakes to solve. Indeed, if Howe and Coser's analysis of the difficult nature of communication between the various base organizations in a CP sound similar to, for instance, the one Althusser (1977) made of the French Communist Party, it is undoubtedly because both grew out of similar concerns.

GLAZER: COMMUNISM AS AN IMPORT FROM EUROPE

Another work on North American communism, which appeared a few years after Howe and Coser's book, is Nathan Glazer's *The Social Basis of American Communism* (1961). The explicit goal of this study is to demolish the myth that communists are psychologically imbalanced and that this imbalance explains their political preference. In opposition to ideas of this type, Glazer argues that communists are quite ordinary people, and that if one wants to understand why

some people have become communists one should look at their social origins. "I believe that in answering 'who [is a communist],' I have also answered many questions about 'why' " (Glazer, 1961:6).

The implicit goal of Glazer's study, however, is somewhat different. His main interest is to make the debate about communism more rational, so that one may combat communism more efficiently, i.e., circumvent hardcore communists rather than liberals or Jews. With this purpose in mind, he focuses on the relationship between communism and ethnicity. His main argument is that insecure status has made certain immigrants vulnerable to communism, but that with increased wealth, hence better conditions for immigrants, the U.S. will have nothing to fear from communism.

In his analysis of the membership of CPUSA Glazer (1961:6–7) focuses on three variables: "class," "race," and "ethnic background." During the 1920s, for instance, Glazer finds that CPUSA was dominated by foreign-born members. Glazer presents empirical material on the so-called "language federations" and tries to analyze their role in the party. He finds that there was little conflict between the party and the federations. Likewise, he discovers very little resistance to the dissolution of the federations when the party was "bolshevized" in the second half of the 1920s. Some people who did not want to become "single-minded soldiers of the revolution" (Glazer, 1961:52) did leave the party, but on the whole "the resistance [was] hardly visible" (1961:70).

During the 1930s the party tried very hard to enroll native workers and especially black people. Glazer documents both these trends, noting that the party was moderately successful with blacks, but tended to attract mostly unemployed native workers. He also documents the influx into the party of people with middle class backgrounds and from the big cities.

Glazer's interest in ethnicity is especially noticeable in the section of the book in which he discusses Jewish communists. Many of the Jewish members in the 1930s and 1940s, Glazer says, were middle class. They felt "resentment" as "latecomers" to certain middle class professions, and

they also came from a liberal-socialist tradition which made them have "lower resistance to communist appeals" (Glazer, 1961:147–148).

Glazer (1961:151) carefully studies the policy of CPUSA toward the Jews as an ethnic group and concludes that ". . . in general there was no group in the population for which the party showed more contempt and disdain in its formulations of specific party positions." The author also notes that Jewish members, despite antisemitism in the CCCP and the hostile silence about Jewish affairs within the party, often chose not to leave the party.

Glazer (1961:188) sums up the general tendency of his work in these words:

> While it [socialist ideology] achieved some partial hold among American intellectuals, its greatest spread was in certain immigrant groups who had learned the Socialist interpretation of historical events in Europe. And it is of this localization of Socialist ideology in America that our analysis has again and again been forced to work in terms of given ethnic groups rather than of other social categories.

Once these immigrant groups were in the U.S., Glazer (1961:191) adds, "democracy" and "wealth" prevented the socialist ideology from spreading.

The strength of Glazer's work resides, in my opinion, in its emphasis on empirical research and on the importance of ethnicity. In both of these areas Glazer breaks new ground in the sociological study of communism. He (Glazer, 1961:11) sums up his notion of the "perfect" research strategy in the following way:

> From the point of view of the sociologist, the perfect material for this study would be a statistically analyzed report based on a fully interviewed cross section of the Communist Party, made at intervals during the forty years of its existence. I believe this kind of data is literally impossible to get, though it might be approximated at great cost from information in the possession of the F.B.I. and the Communist Party.

How sophisticated an analysis Glazer actually could make using this method, however, is open to doubt. And doubt increases when one takes a closer look at Glazer's idea of the relationship between ethnicity and communism. To view radicalism as an essentially European phenomenon exported to the U.S. is to ignore the fact that working class radicalism (including its various political expressions) has been structural to all industrialized countries since the 19th century. It also restricts the study of socialism and communism in the U.S. to measuring various ideological "influences" of Europe rather than focusing on the social structure in the U.S. Moreover, with his strong tendency to view social phenomena in terms of "ethnicity" (as opposed to class), Glazer is, unable to go beyond a certain point in his analysis of communism.

Glazer's conclusion is that a capitalist society can take care of communism with the help of "wealth" and "democracy." We think it is reasonable to take this statement as a sign that a definite hostility to communism lies behind Glazer's study, but one that wants to caution rather than coerce. In short, his study is like Stouffer's a plea for a more enlightened attitude to the "communist danger."

LIPSET: COMMUNISM AS WORKING CLASS AUTHORITARIANISM

In 1960, S. M. Lipset's *Political Man* appeared. In this work, which is one of the most celebrated in North American sociology, an analysis of communism plays a central role.[9] As with Selznick and Shils, what structures Lipset's analysis is the strong conviction that communism is an extremely dangerous threat to democratic society.

The most important political issue in our time, Lipset (1960:341) argues, is that of "freedom versus communism." Communism is seen as the "main threat to freedom" (1960:341). Particularly the working class is susceptible to communism since it consists of predominantly authoritarian people. "Working class authoritarianism" is due to poor education, economic insecurity, and similar causes. Working class people thus have a general

---

[9] In Lipset (1958), communism is analyzed in greater detail.

tendency to make simplified choices in political life (1960:ch.4).

After having set down these basic principles for his analysis, Lipset tries to apply his notion of "working-class authoritarianism" to the development of the communist parties in Western Europe. In countries with large parties, he argues, people vote communist because it represents the simplest alternative. In countries with small communist parties, the average voter tends to vote labor/socialist as the easiest alternative. In these latter countries, voting communist represents a more complex choice, something which only higher income workers are capable of doing. Lipset uses income and voting behavior to support this analysis of communism in Western Europe.

Political Man also contains a discussion of communism in parts of the world other than Europe. On the whole Lipset argues that the development of communism has to be understood within the process of "modernization." Thus, he posits an "inverse relationship" between national income and the spread of communism (1960:62–63). Lipset modifies his position by saying that it is more the coexistence of high and low incomes that creates the potential for communism than poverty itself ("relative deprivation"). Hence he forecasts that a non-industrialized country will have no communism, a developing country—especially if industrialization proceeds quickly—will have a strong radical movement, and a fully modernized society will have little communism.

Lipset's study, no doubt, represents a step forward in the sociological study of communism in that the author attempts to make a comparative study of communism. In other aspects, however, Lipset's study represents a regression especially in relation to Howe and Coser's study. Lipset ignores certain obvious factors, which is surprising considering his general sophistication. In my opinion these omissions result from his fervent anti-communism.

Apart from the shaky social-psychological basis of Lipset's argument, there are other, equally severe drawbacks to his perspective. Lipset ignores, for instance, that communist support among workers is historically determined by

more factors than income. He also fails to take into consideration that the organizational form of communism—the party—has a fairly autonomous impact.

The importance of the first objection—that pro-communism among the workers is not determined by income alone—can be seen in the critique Walter Korpi made of Lipset in 1971. Korpi (1971:971–984) effectively criticized Lipset by showing that the data used in *Political Man* to prove that different types of workers support large and small communist parties in Western Europe can be explained better in terms of whether these supporters live in the city or in rural areas.

Lipset's tendency to ignore the importance of the party as an organization is also a fault of his study, and it represents a step backward from Howe and Coser. By analyzing the motivations of the leadership apart from those of the working class members, Howe and Coser avoided the usual explanation of communism as a form of "authoritarianism." This distinction between the leadership and working class members, however, is not to be found in *Political Man*. Neither can one find any inkling that the international communist movement was by no means monolithic in 1960. Here, as elsewhere, Lipset's anti-communism makes him omit the study of factors which are important to consider in an analysis of communism.[10]

Lipset's second argument, that communism can be stopped by increased wealth, also suffers from several fundamental weaknesses. He assumes, for instance, a direct relationship between economic development and the strength of communism. This too is simplistic. That Lipset's thinking on this point is weak has been shown quite convincingly by Marsh and Parish (1965:934–942).

There also is Lipset's virtual omission of the role of the state in relation to communism. To argue, for instance, that CPUSA is small because the U.S. is a rich

---

[10] In a paper given at a conference on "Changes in Soviet Society," at Oxford, England, June 1957, Daniel Bell (1965:349) argued: "In fact, not monolithism but factionalism has been a basic law of the Communist movement. And we have failed to see this and exploit it."

country is to bypass too quickly the suppression of CPUSA by the American state.

The most bothersome aspect about Lipset's contention that communism will be "modernized away," however, is that it more or less amounts to saying that communism stands outside the main trends of history and hence is not of much interest, scholarly or otherwise. "The fundamental political problems of the industrial revolution have been solved" is a famous quotation from *Political Man* (1960:406), and it summarizes this attitude. In a later article, Lipset (1968:241) makes no bones about the residual character of communism in Western Europe by speaking of it mainly as a type of "resistance to modernization."[11]

CONCLUSION

This overview has shown how the various works on communism in mainstream American sociology have been shaped by the way the topic was initially conceived.[12] For Selznick, Shils, and Lipset, communism presented itself as something intrinsically evil that must be combatted. Bell saw communism as a dangerous and utopian folly; Stouffer's (and perhaps Glazer's) overriding concern was to make the distinction between liberalism and communism clear in order to protect liberals from right-wing attacks. Howe and Coser were interested in separating democratic socialism from Stalinist communism to save the former from the wrath of anticommunists. These concerns, I suggest, underlie the authors' choice of focus on different aspects of communism and also account for why, to a large extent, we

have empirical facts on certain aspects of communism and not on others. It also accounts for the one-sided and often low quality of the research: hostility to the topic often blinded these researchers to fairly obvious facts that they would not have ignored if they had researched a different political party, for instance, or a different social movement.[13]

What role did the "sociological tradition" play in the way these studies were structured? On the whole, I think that it did not play a central role. Selznick et al. used the sociological theory and the method they were familiar with to cast light on communism, but neither their theory nor their methods accounts for the general thrust of the studies that resulted. To take one example: Coser and Shils both work in the structural-functional tradition; nevertheless their analyses move in quite different directions along the lines of their differences in sympathy for the left. In short, the professional knowledge that was available was adjusted to suit the more fundamental interests of the scholars.

Given this situation, the Hegelian-Marxian approach, with its emphasis on the sociologist as an organic part of the unfolding of society, seems more congenial than the "sociological tradition" to an understanding of the way sociological analysis is generated. This conclusion, however, should not be seen as suggesting that the Hegelain-Marxian tradition, as it exists today, can solve all the problems

---

[11] See especially the section called "Communism resists the Trend."

[12] This is also true of the sociological studies of communism that come from scholars with a New Left-perspective, such as Ross (1975, 1978), Ross and Jensen (1979), and Zeitlin (1970). Both Ross and Zeitlin are mainly concerned with working class-radicalism, and this has made their research focus on very different questions from those posed in mainstream sociology. In our opinion, their studies are of superior quality to those that we have reviewed here. Ross has studied the French Communist Party over a long period of time and is now in the process of publishing several major studies of it.

[13] Selznick and Shils have been involved in anticommunist activities according to *Who's Who in America* (1968:2920) and Bell (1965:317, 428). In the pamphlet *National Support for Behavioral Science* (1958:1) several prominent sociologists—including Stouffer and Merton—argue that efforts should be taken to insure that "the national interest be adequately served by the development and application of behavioral science" in order that the Soviet Union will not catch up. For information on the relationship between U.S. social scientists and anti-communism, see, for example, John M. Crewdson, "Worldwide Propaganda Network Built and Controlled by the C.I.A.," *New York Times*, December 26, 1977, and McAuliffe's (1978:114–129) as well as Lasch's (1969:61–114) account of the American Committee for Cultural Freedom. The anti-communism of social scientists in the 1960s and the 1970s has been extremely well documented by Chomsky and Herman (Chomsky, 1969:23–158; Chomsky and Herman, 1979).

involved in this question. The major social forces are perceived by sociologists in extremely intricate ways, and generate an intellectual product by means that we, at present, know very little about. If we are to understand the role of the social scientist in society, it is imperative to focus attention on the cognitive aspects of this process and to try to develop some theoretical schemes that can do full justice to the complicated generative processes involved in the production of social theories. The relationship between a scholar's political commitment and their scholarly work is one which, in addition to having a moral dimension, has a cognitive one. To paraphrase Pascal: The mind has its reasons that the heart doesn't know.

REFERENCES

Adler, Les K. and Thomas G. Peterson
1970 "Red fascism: The merger of Nazi Germany and Soviet Russia in the American image of totalitarianism, 1930's-1950's." American Historical Review 75:1046-1064.

Almond, Gabriel
1954 The Appeals of Communism. Princeton: Princeton University Press.

Althusser, Louis
1978 "What must change in the party." New Left Review 109:19-45.

Bauer, Raymond A., Alex Inkeles, and Clyde Kluckhohn
[1956] How the Soviet System Works: Cultural,
1961 Psychological, & Social Themes. Cambridge: Harvard University Press.

Becker, Howard S.
1967 "Whose side are we on?" Social Problems 14:239-247.

Bell, Daniel
[1952] Marxian Socialism in the United States.
1967 Princeton: Princeton University press.
1955 (ed.) The New American Right. New York: Criterion Books.
1965 "Ten theories in search of reality: The prediction of Soviet behavior." Pp. 315-353 in The End of Ideology: On the Exhaustion of Political Ideas in the Fifties. Revised Edition. New York: The Free Press.

Berger, Peter L.
1971 "Sociology and freedom." American Sociologist 6:1-5.

Blackmer, Donald L. M. and Sidney Tarrow (eds.)
1975 Communism in Italy and France. Princeton: Princeton University Press.

Brown, Roger
1965 Social Psychology. New York: The Free Press.

Cantrill, Hadley
1958 The Politics of Despair. New York: Basic Books.

Chomsky, Noam
1969 "Objectivity and liberal scholarship." Pp. 23-158 in American Power and the New Mandarins. New York: Random House.

Chomsky, Noam and Edward S. Herman
1979 The Political Economy of Human Rights, I-II. Boston: South End Press.

Clark, Terry Nichols
1973 Prophets and Patrons: The French University and the Emergence of the Social Sciences. Cambridge: Harvard University Press.

Coser, Lewis
1974 Greedy Institutions: Patterns of Undivided Commitment. New York: The Free Press-Collier Macmillan.

Crewdson, John M.
1977 "Worldwide propaganda network built and controlled by the C.I.A." The New York Times (December 26):1, 37.

Davis, Jerome
1930 Contemporary Social Movements. New York: The Century Company.
1933 (ed.) The New Russia: Between the First and Second Five Years Plans. New York: John Day Company.

Denzin, Norman K.
1970 "Who leads: Sociology or society?" American Sociologist 5:125-127.

Elliot, P. and P. Schlesinger
1979 "On the stratification of political knowledge: 'Eurocommunism,' an unfolding ideology." Sociological Review 27:55-81.

Friedrichs, Robert W.
1968 "Choice and commitment in social research." American Sociologist 3:8-11.
1970 "Epistemological foundations for a sociological ethic." American Sociologist 5:138-141.

Glazer, Nathan
1961 The Social Basis of American Communism. New York: Harcourt, Brace & World, Inc.

Glazer, Nathan and Seymour M. Lipset
1955 "The polls on communism and conformity." Pp. 141-165 in Daniel Bell, ed., The New American Right. New York: Criterion Books.

Goldmann, Lucien
1950 Sciences Humaines et Philosophie. Paris: Presses Universitaires de France.

Gorin, Zev
1979 "Socialism and dependency in Parsons' theory of evolution." Paper presented at the 74th Annual Meeting of the American Sociological Association, August 27-August 31, 1979, Boston.

Gouldner, Alvin W.
1962 "Anti-Minotaur: The myth of value-free sociology." Social Problems 9:199-213.
1968 "The sociologist as partisan: Sociology and the welfare state." American Sociologist 3:103-116.

Howe, Irving and Lewis Coser
1957 The American Communist Party: A Critical History (1919-1957). Boston: Beacon Press.

Inkeles, Alex
    1950a Public Opinion in Soviet Union: A Study in
           Mass Persuasion. Cambridge: Harvard
           University Press.
    1950b "Social stratification and mobility in the
           Soviet Union." American Sociological Re-
           view 15:465–479.
    1968   Social Change in Soviet Union. Cambridge:
           Harvard University Press.
Inkeles, Alex and Raymond A. Bauer
    1959   The Soviet Citizen: Daily Life in a Totalita-
           rian Society. Cambridge: Harvard Univer-
           sity Press.
Korpi, Walter
    1971   "Working class communism in Western
           Europe: Rational or nonrational?" Ameri-
           can Sociological Review 36:971–984.
Kuhn, Thomas S.
    [1962] The Structure of Scientific Revolutions.
    1970   Second edition, enlarged. Chicago: Univer-
           sity of Chicago Press.
Lasch, Christopher
    1969   "The cultural Cold War: A short history of
           the Congress for Cultural Freedom." Pp.
           61–114 in The Agony of the American Left.
           New York: Knopf.
Lasswell, Harold and Dorothy Blumstock
    1939   World Revolutionary Propaganda: A
           Chicago Study. New York: Knopf.
Lasswell, Harold, Nathan Leites, and Associates
    1949   Language of Politics: Studies in Quantita-
           tive Semantics. Cambridge: The MIT Press.
Lazarsfeld, Paul
    1955   The Academic Mind: Social Scientists in a
           Time of Crisis. Glencoe, IL: The Free
           Press.
Lipset, Semour M.
    1958   "Socialism-left and right-East and West."
           Confluence 7:173–192.
    1960   Political Man: The Social Bases of Politics.
           New York: Doubleday.
    1968   Revolution and Counterrevolution: Change
           and Persistence in Social Structures. New
           York: Basic Books.
Lipset, Seymour M. and Earl Raab
    1970   The Politics of Unreason: Right-Wing Ex-
           tremism in America, 1790–1970. New
           York: Harper & Row.
Lukács, Georg
    [1954] Die Zerstoerung der Vernuft, I-III.
    1974   Darmstadt u. Neuwied: Luchterhand.
McAuliffe, Mary Sperling
    1978   Crisis on the Left: Cold War Politics and
           American Liberals, 1947–1954. Amherst,
           MA: University of Massachusetts.
MacRae, Duncan Jr.
    1971   "A dilemma of sociology: Science versus
           policy." American Sociologist 6 (Sup-
           plementary Issue):2–7.
Marsh, Robert M. and William L. Parish
    1965   "Modernization and communism: A re-test
           of Lipset's hypotheses." American
           Sociological Review 30:934–942.
Mead, Margaret
    1951   Soviet Attitudes Toward Authority: An In-
           terdisciplinary Approach. New York:
           McGraw-Hill's RAND-Series.

Mills, C. Wright
    1953   Review of Philip Selznick, The Organ-
           izational Weapon. American Journal of
           Sociology 58:529.
    1958   The Causes of World War Three. New
           York: Ballantine Books.
    1961   The Marxists. New York: Dell Publishing
           Co.
Moore, Barrington, Jr.
    1945   "The communist party of the USA: An
           analysis of a social movement." American
           Political Science Review 34:31–41.
    1950   Soviet Politics: The Dilemma of Power.
           Cambridge: Harvard University Press.
    1954   Terror and Progress: USSR. Cambridge:
           Harvard University Press.
    1958   National Support for Behavioral Science.
           No place: no publisher.
Mullins, Nicholas C.
    1973   Theories and Theory Groups in Contempo-
           rary American Sociology. New York:
           Harper & Row.
Nicolaus, Martin
    1969   "Remarks at the ASA Convention."
           American Sociologist 4:154–156.
Parsons, Talcott
    1964   "Communism and the West: The sociology
           of conflict." Pp. 390–399 in Amitai Etzioni
           and Eva Etzioni (eds.), Social Change:
           Sources, Patterns, and Consequences. New
           York and London: Basic Books.
Record, Wilson
    1951   The Negro and the Communist Party.
           Chapel Hill: University of North Carolina.
Ross, George
    1975   "Party and mass organization: The chang-
           ing relationship of PCF and CGT." Pp.
           504–540 in Donald L.M. Blackmer and Sid-
           ney Tarrow (eds.), Communism in Italy and
           France. Princeton: Princeton University
           Press.
    1978   "Crisis in Eurocommunism: The French
           Case." Pp. 172–193 in Ralph Miliband and
           John Saville (eds.), The Socialist Register
           1978. London: Merlin Press.
Ross, George and Jane Jensen
    1979   "Strategies in conflict: The twenty-third
           congress of the French Communist Party."
           Socialist Review 9:71–99.
Schwendinger, Herman and Julia R. Schwendinger
    1974   The Sociologists of the Chair: A Radical
           Analysis of the Formative Years of North
           American Sociology (1883–1922). New
           York: Basic Books.
Selznick, Philip
    1952   The Organizational Weapon: A Study of
           Bolshevik Strategy and Tactics. New York:
           McGraw-Hill's RAND-Series.
Shils, Edward
    1954a "Authoritarianism: 'Right' and 'left'." Pp.
           24–49 in Richard Christie and Marie
           Jahoda (eds.), Studies in the Scope and
           Method of "The Authoritarian Personal-
           ity." Glencoe, IL: The Free Press.
    1954b The Torment of Secrecy: The Background
           and Consequences of American Security
           Politics. Glencoe, IL: The Free Press.

Smith, Dusky Lee
[1964] "The sunshine boys: Towards a sociology of happiness." Pp. 371–387 in Larry T. Reynolds and Janice M. Reynolds (eds.), The Sociology of Sociology. New York: McKay Company.
Sorokin, Pitirim A.
1944 Russia and the United States. New York: E. P. Dutton and Company.
1966 Sociological Theories of Today. New York: Harper & Row.
Stacey, Barrie
1978 Political Socialization in Western Society: An Analysis from a Life-Span Perspective. London: Edward Arnold.
Stouffer, Samuel
1955 Communism, Conformity and Civil Rights: A Cross-Section of the Nation Speaks Its Mind. New York: Doubleday.
Timasheff, Nicholas S.
1946 The Great Retreat: The Growth and Decline of Communism in Russia. New York: E. P. Dutton and Company.
Vogel, Ezra F.
1969 Canton under Communism: Programs and Politics in a Provincial Capital. Cambridge: Harvard University Press.
Watnick, Morris
1953 "The appeal of communism to the peoples of underdeveloped areas." Pp. 428–436 in Reinhard Bendix and Seymour Martin Lipset (eds.), Class, Status and Power: A Reader in Social Stratification. Glencoe, IL: The Free Press.
Weber, Max
(1919) "Politics as a vocation." Pp. 77–128 in H.
1946 H. Gerth and C. Wright Mills (eds.), From Max Weber: Essays in Sociology. New York: Oxford University Press.
Who's Who in Amerca
1968 "Philip Selznick." (P. 2920) Chicago: Marquis Who's Who.
Yang, C. K.
1965 Chinese Communist Society: The Family and the Village. Forward by Talcott Parsons. Cambridge: The MIT Press.
Zeitlin, Maurice
1970 Revolutionary Politics and the Cuban Working Class. New York: Harper & Row.

SOCIOCULTURAL VS. BIOLOGICAL/SEXIST EXPLANATIONS OF SEX DIFFERENCES IN CRIME: A SURVEY OF AMERICAN CRIMINOLOGY TEXTBOOKS, 1918–1965*

DARRELL J. STEFFENSMEIER AND ROBERT E. CLARK

The Pennsylvania State University

In this report, we provide a survey of how American authors of traditional ("older") criminology textbooks explain sex differences in crime. The survey includes all texts published during the period from 1918, when the first text was published, to 1965. In contrast to the claims of some contemporary writers, we found little evidence of a biological or sexist bias in the textbook treatment of sex differences in crime. Rather, as a general rule, the authors present a sociogenic interpretation of sex differences in crime, emphasizing the role of structural and cultural variables and avoiding biological hypotheses and the like. Our review demonstrates more continuity in the developmental processes involved in criminological thought than is sometimes suggested in contemporary writings.

This paper is an excursion into the sociology of criminology, providing a historical overview of the way in which criminologists have traditionally explained the universal finding that males are more heavily involved in crime than females. The existing commentaries commonly claim that a biological (and sexist) bias exists in the older formula-

tions in explaining sex differences in crime rates and female criminality in general. These commentaries, however, tend to be somewhat cursory and selective reviews. This paper is designed to fill in some historical gaps. More specifically, the paper deals with how authors of criminology textbooks have explained sex differences in criminality. It is concluded that rather than a biological bias in the textbooks, there exists a preponderant sociological or sociocultural bias.

---

* Address all communication to: Darrell J. Steffensmeier, Department of Sociology, The Pennsylvania State University, University Park PA 16802.

BACKGROUND

Two assertions are often made in contemporary reviews of the literature on female crime. One is that female criminality has been ignored in large part by criminologists and social scientists (Smart, 1976; Simon, 1975; Wilson and Rigsby, 1975; Harris, 1977; Bowker, 1978). The second is that traditionally most writers on the subject have traced female criminality to biological or psychological sources, with little or no attention being given to the role of social-structural factors (Millman, 1975; Klein, 1973; Simon, 1975; Smart, 1976). The first claim is interpreted by some as evidence that the literature is "sexist," i.e., the lack of scientific information on female criminality is viewed as just another symptom of the "unimportant" status of women in an unequal society (Millman, 1975; Rasche, 1974).

The severest criticism of the traditional literature, however, is that the biological determinism evident in the classical works of female criminality—e.g., of Lombroso and Freud—has created (and reflects) an ideological framework on which later, more contemporary studies have developed. Dorie Klein, in her oft-cited commentary on the literature, traces criminological theorizing about female crime to Lombroso:

> These assumptions of universal, biological/ psychological characteristics, of individual responsibility for crime, of the necessity for maintaining social harmony, and of the benevolence of the state link different theories along a continuum, transcending political labels and minor divergences. The road from Lombroso to the present is surprisingly straight (Klein, 1973:6–7).

Rita Simon (who relies heavily on Klein's review) reaches a similar conclusion:

> Traditionally most writers on the subject women in crime have traced female criminality to biological and/or psychological sources, with little or no discussion of such social-structural considerations as the state of the economy, occupational and educational opportunities, divisions of labor based on sex roles, and differential association (Simon, 1975:4).

There are several reasons for skepticism regarding this most recently "received interpretation" of the traditional literature on female crime.[1] First, the selective emphasis in these reviews or commentaries may in part reflect a tendency of some feminist writers to find sexist sin almost everywhere. To present the works of Lombroso, Freud, and the Gluecks as representative of traditional criminological thinking of female criminality is a questionable exegesis of the history of American criminology. In this regard, the conclusion that the theories of Lombroso and Freud have a biological bias, and therefore a sex bias which is reflected in later works, bears more on the sociology of criminology and less on the central questions of female crime itself. After all, their biological determinism was not limited to explaining female crime but crime generally—including that of males.

Further, the reviews ignore the extent to which perhaps the large majority of criminologists have rejected the views on female crime of writers such as Lombroso, Freud, and the Gluecks. The perspectives of the latter have been accepted only with important reservations or, at least in the case of Lombroso, hardly accepted at all (see, Gibbons, 1979). The same is true, for example, of Pollak's biologically-based interpretation of the "masked" nature of female crime (see, e.g., Elliott, 1952; Mannheim, 1965). Contrary to claims and suggestions of some commentators (e.g., Klein, 1973; Smart, 1976), it is difficult to identify specific criminological writings in which there

---

[1] Besides the reasons listed here, from an experiential point of view, the conclusions of Klein, Simon, etc., are at odds with our own accumulated impressions. Together we cover a considerable portion of the criminological history: the senior author covers the modern era having received his Ph.D. from the University of Iowa in 1972 while the junior author, now retired from university teaching, received his Ph.D. from the University of Chicago in 1947. Both of us report parallel experiences: in our graduate training and in interactions over the years with other social scientists and criminologists, we found an overwhelmingly strong support for the sociological position on sex differences in crime and a general rejection of biological interpretations— including specific reservations and rejection of the views of writers such as Lombroso, Freud, and the Gluecks.

exists acceptance of Pollak's position that females' deceit and their hidden criminality are in part linked to passivity and other aspects of female sexuality. Rather, the more *sociological* views of Pollak regarding female crime are likely to be cited approvingly, that is, as being an outgrowth of ordinary social roles and feminine role expectations acquired through sexual socialization (Sutherland and Cressey, 1960; Tappan, 1960; Elliott, 1952).

Interrelated with this, the reviews tend to obscure the central questions on female crime. It is not clear whether the commentators are dealing with differences in criminality among women or with sex differences in crime rates. For example, a major heading in Simon's work is: "Why do Women Commit Crime?" Yet the data presented are limited largely to arrest statistics for males and females, and hence the data actually give rise to a different question of "Why are women arrested less often than men?"

A third shortcoming is that the existing reviews largely have ignored the treatment of female criminality as manifest in criminology textbooks. The textbook view of crime causation among women and of sex differences in crime remains largely unknown, although some reviews have dealt with the issue of crime causation (e.g., Millman, 1975; Wilson and Rigsby, 1975). That the textbook view on sex differences in crime has been overlooked is unfortunate. First, textbooks represent to some extent the "state of the discipline" in regard to a topic or issue and tend to "include only knowledge that has achieved acceptance in the field" (Rothman, 1971:127). Second, textbooks serve to channel theoretical views and empirical findings to consumers—future criminologists and social scientists, practitioners, and lay persons in general (Inkeles, 1964). Third, textbooks reflect the interests and activities of scholars in the field (Mills, 1943).

.The purpose of this report, therefore, is to examine explanations of sex differences in criminality as they are manifest in 34 criminology textbooks, written by 22 authors, covering the period of 1918 to 1965. The year of 1918 marks the publication of the first criminology textbook, authored by Maurice Parmelee (see, Gibbons, 1974). Textbooks published since 1965 were not included in our review because we were concerned with how textbook authors "traditionally" explained sex differences in crime. Further, a review of sorts of *modern* criminology textbooks has been provided by Wilson and Rigsby (1975). They conclude that a sexist and biological bias exists in criminology textbooks, even though interpretations of sex differences in crime tend to have a sociological rather than a biological emphasis. The overlap that might exist between Wilson and Rigsby's and the present review is counterbalanced by some disagreement in characterization of textbook authors' views. For example, Wilson and Rigsby list Sutherland as favoring a structural explanation whereas we list the early Sutherland as being more of a culturalist and the later Sutherland as favoring a combination of structural and cultural variables. Other differences between this and the Wilson and Rigsby review are noted later.

We also examine the amount of space textbooks devote to the subject of *age* differences, relative to that devoted to sex differences. Rather than viewing the lack of textbook discussion of female criminality as reflecting a sexist bias, it may reflect the belief that crimes by women are so rare that female criminality and sex differences in crime can be ignored without altering the outcome. In this regard, it is also widely believed that youth are much more likely to be involved in crimes than the middle-aged or elderly. On the assumption that analysts, for *practical* reasons, will pay little attention to that part of the social landscape producing the lowest criminal yield—females and older persons—we should expect sparse treatment of *both* sex and age differences. If textbook space devoted to age differences is more than that devoted to sex differences, therefore, we interpret this as evidence of a sexist bias. On the other hand, if the coverage of age differences is less or equal to that of sex differences, we infer from this that there is not a sexist bias. (Still, we concede the possibility that both a sexist and an ageist bias exist.)

RESULTS OF TEXTBOOK SURVEY

Thirty-four textbooks, written by 22 authors/coauthors, are included in our survey. To our knowledge, this survey is exhaustive in that it includes all criminology textbooks published during the 1918–1965 period.[2] In those instances where there are several or more editions of a textbook (e.g., Sutherland, Reckless), we elected to include only a representative sample of editions.

Twenty-three of the textbooks we examined contained some reference to the much lower official rates of crime among women as compared to men, and offered some explanation of these differences. Two textbooks made reference to the lower rates of female crime but offered no explanation (Ettinger, 1932; Tappan, 1960). Nine textbooks did not discuss differences in male and female crime rates and also, of course, offered no explanation. The latter are the "older" textbooks, published mostly in the 1920s and 1930s; these texts in large part devoted little space to explaining crime in general but rather emphasized penology and criminal justice materials.

An outstanding exception to this "older" textbook pattern is that of Maurice Parmelee's treatment of sex differences in crime, which we examine closely here. In this, the *first* criminology textbook published in 1918, Parmelee provides the most systematic textbook treatment of the issue.[3] A closer look at Parmelee's discussion also permits comparison to contemporary treatments of sex differences in crime and provides a yardstick for assessing the development of criminological thought on the subject.

After presenting statistical evidence on patterns of male and female criminality,

Parmelee first discusses what he considers to be the less significant, and even invalid, explanations of sex differences in crime. He specifically rejects the argument that the female (sex) is innately more moral than the male (sex):[4] "We have no reason to believe that woman has innate traits which render her more moral" (1918:242). But, Parmelee does suggest (similar to Elliott below) that maternity may have moral significance and enhance the sympathetic, emotional traits associated with womanhood.

Similar to several other textbook authors, Parmelee subscribes to the view that physical differences help explain sex differences in crime.

It is evident, to begin with, that woman's inferiority in physical strength shuts her out almost entirely from many kinds of crime requiring great physical strength, such as burglary, highway robbery, various forms of murder, etc. Furthermore, the relatively passive role of the female in sexual intercourse makes it almost impossible for her to commit certain kinds of sexual crimes, such as rape, however, strong may be her desire to commit these crimes (1918:240).

Parmelee does depart, however, from subsequent textbook authors (with the possible exception of Reckless) in his adoption of prevailing views about the greater *variational tendency* of males to explain sex differences in crime.

It is highly probable that the male sex is more variable. The significance of this greater variability for our purpose is that the male sex probably varies more than the female sex in certain directions, which lead to crime . . . Furthermore, it is frequently alleged that the male is more katabolic, the female being relatively anabolic. That is to say, the male is said to be more active and initiative, thus expending energy more freely, while the female is said to be more passive and to be storing up energy (1918:242–243).

---

[2] We do not include in our "exhaustive" survey textbooks of a parochial or religious perspective (e.g., Wiers, 1941), nor textbooks which are concerned exclusively with demonstrating that crime is due generally to glandular, neurological, etc., abnormalities (e.g., see, Schlapp, 1928).

[3] Prior to Otto Pollak's, *Criminality of Women* (1950), Parmelee's is perhaps the most systematic and comprehensive discussion of sex differences in crime available in the literature. Many of the ideas in Pollak's work, in fact, are predated in Parmelee's treatment.

---

[4] Some contemporary analysts of female crime (e.g., Simon, 1975; Adler, 1975) are quick to note that women are no more moral than men. This enlightened view is really a "straw man" vis-à-vis traditional writers on the subject. While some 19th century writers, such as Gabriel Tarde, advanced the moral superiority position, it is difficult to concretely identify a 20th century analyst who did so.

Parmelee is absolutely clear, nevertheless, in his conclusion that the principal cause of sex differences in crime is *sociocultural:* "The woman is shielded from criminality by her secluded life." He also concludes that the apparent or officially lower criminality of women is in part due to "lenient treatment of female criminals."

We come now to the *two* principal causes for the *apparently* lower criminality of women [emphasis ours]. The *first* is that women obtain much fewer opportunities to commit crimes than men. Woman's sphere of activities has almost invariably been within the home, frequently secluded from the outer world. Up to the present time they have not taken part to any great extent in the economic occupations and the professions outside of the home. They have not been subjected to the same extent as men to the bitter economic struggle for existence, which is borne for them in part by the men. Occupied within the home with their household and maternal duties they have been shielded from many temptations, from many corrupting influences, and to a large extent from alcoholic stimulation . . . It is, therefore, to be expected that as woman's position becomes more like that of man her criminality will increase . . . This does not mean necessarily that her criminality will ever reach that of man, however much her social position may become like that of man, for there will always remain the innate physical and mental differences between the sexes which tend to depress the relative criminality of woman (1918:244–245).

The *second* great reason for the apparently lower criminality of women is that there are many more extra-judicial female crimes than there are extra-judicial male crimes. That is to say, there are many more crimes committed by women which are not recorded in the judicial statistics than there are of unrecorded crimes committed by men. This is due partly to the favoritism shown to women which is mentioned above. But it is due principally to the fact that female crimes are more difficult to discover than male crimes (1918:245).

In sum, Parmelee is expressing essentially a sociocultural position, although he does hold that sex differences in crime are due partly to sex differences in physical strength, levels of passivity/activity, and variational tendency. The latter, of course, reflect the widespread view of

Parmelee's contemporaries such as Havelock Ellis (1914) and W. I. Thomas (1907). Further, while notions of variational tendency and male as katabolic vs. female as anabolic are rejected in large part by analysts today, many contemporary sex role researchers do conclude that males are "naturally" more aggressive, have higher metabolism rates, and higher activity/energy levels than do females (Weitz, 1977; Kando, 1978).

Like Parmelee, subsequent textbook authors have leaned heavily toward a sociological interpretation of sex differences in crime. Table 1 shows the explanations offered by each author. An Xx indicates which explanations were given: the large X indicates our impression of the explanation each author favored or gave primary emphasis; the small x indicates the explanation given secondary emphasis. A check (✓) indicates that *no* explanation was given by the author. And, an asterisk (*) indicates whether the author held that sex differences in *official* crime are at least in part due to differential treatment of male and female suspects. At the far right side of Table 1 is shown the number of pages that each author devoted specifically to a treatment of sex as well as age differences in crime.

Table 1 reveals that in nearly all criminology textbooks, the authors provide a *sociogenic* explanation of sex differences in crime and avoid or even reject biological hypotheses and the like. The sociogenic view links the lower rate of female criminality to structural and cultural aspects surrounding women's location in society.[5] Several authors leaned toward a *structural* explanation, pointing

---

[5] In attempting to classify a particular author as leaning more toward a structural or cultural interpretation of sex differences in crime, we were made acutely aware of the difficulty of maintaining even an analytical distinction between the concepts of social structure and culture. Our task would have been easier had we followed those sociologists who suggest combining the two concepts on the grounds that all human phenomena are truly sociocultural. Nevertheless, we attempted to distinguish the two concepts, thinking of social structure as a relatively stable arrangement or set of social relations, and of culture as referring to norms, knowledge, beliefs, and attitudes which are passed down from generation to generation (see, Theodorson and Theodorson, 1969).

TABLE 1. Treatment of Sex Differences in Crime via American Criminology Textbooks, 1918–1965

	Sociocultural		Physical Differences (Strength)	Inherent Biological/ Psychological Differences (aggressiveness, passivity)	Differential Treatment	No explanation	Pages Devoted To Sex Differences	Pages Devoted To Age Differences
	Structural (Opportunity)	Cultural (Socialization)						
Parmelee, 1918	X	X	X	X	*		17	11
Sutherland, 1924		X			*		1	3
1934	X	X			*		2	4
1947	X	X			*		3	4
(& Cressey) 1960	X	X			*		5	4
Parsons, 1926						√	—	—
Gillin, 1926						√	—	—
1945	X	X					3	7
Gault, 1931	X	X	X				4	3
Ettinger, 1932							3	2
Morris, 1934						√	—	4
Haynes, 1935						√	—	—
Tannebaum, 1938						√	—	—
Cantor, 1939						√	—	—
Reckless, 1940	X	X		X	*		7	6
1950	X	X		X	*		3	4
1961	X	X		X	*		3	3
Wood & Waite, 1941	X	X	X		*		4	7
Taft, 1942	X	X	X		*		1	1
1950	X	X	X		*		3	2
(& England) 1964						√	—	—
Barnes & Teeters, 1943	X	X	X		*		4	—
1951						√	—	—
1959	X	X	X		*		3	3
Cavan, 1948	X	X					7	1
1962	X	X			*		7	4
Elliott, 1952	X	X			*		13	4
Caldwell, 1956	X		X		*		2	2
1965	X	X	X		*		1	1
Korn & McCorkle, 1959	X	X					3	1
Tappan, 1960						√	2	3
Bloch, 1962	X	X					3	3
Johnson, 1964	X	X	X		*		6	5
Hartung, 1965						√	—	—
Total							106	92

out the limited opportunities open to women as opposed to men, and the added protection offered women in the form of a more sheltered life style. Most authors, however, gave equal prominence to *cultural* aspects, speaking in terms of sex role expectations and of sex differences in socialization and the learning of attitudes and behaviors. None of the authors favored a cultural over a structural explanation, with the exception of the early Sutherland who in his 1924 text gave this brief explanation of sex differences in crime rates:

Such comparisons make it clear that many elements of a social nature are involved— codes, standards, ideals—which affect the sexes unequally, and that the different rates are not due to sex traits as such (1924:93).

In subsequent textbook publications, Sutherland's cultural learning view is expanded and a structural perspective is added.

It has been shown that maleness is not significant in the causation of crime in itself, but only as it indicates social position, supervision, and other social relations. The significant difference is that girls are supervised

more carefully and the behavior in accordance with social codes taught them with greater care and consistency than is the case with boys. From infancy girls are taught that they must be nice, while boys are taught that they must be rough and tough; a boy who approaches the behavior of girls is regarded as a 'sissy' (1947:101).

Along with the later Sutherland, other textbook writers who offered an explanation of sex differences in crime did so by emphasizing elements of both the structural and the cultural view. (Reckless is a possible exception.) While the explanations were in large part *sociocultural,* it was not easy in some instances to differentiate whether the author leaned toward a structural or a cultural interpretation. In this regard, in addition to labeling the early Sutherland as favoring the cultural viewpoint, we classified four authors as favoring the structural perspective (Parmelee, 1918; Gault, 1931; Taft, 1942, 1950; Caldwell, 1956). An example is that of Caldwell in his 1956 text (but compare to the 1965 edition) where he writes:

> . . . the traditions and customs of our society [to] produce the differences between the sexes in crime and delinquency. Despite her increased freedom, the average female in the United States is still more closely supervised and protected than the male, is shielded from much of the harsh conflict and competition of the business world, is concerned primarily with domestic affairs, and is given preferential treatment in many social relationships. As a result, she is placed in fewer situations that are conducive to crime and delinquency . . . (Caldwell, 1956:199–200).

Most of the authors, however, presented an approximately *equal* mix of structural and cultural variables, as reflected in the earlier quote from Sutherland (1947:101) and in this statement from Gillin (1945:53).

> The differences between the sexes are probably due to variations in occupation, in the social role assigned to the sexes, and to the differences in training.

A final example is that of Wood and Waite (1941:238), who also touch base with additional views regarding sex differences in crime:

It is, of course, the relative physical weakness of women which precludes their extensive participation in the more violent predatory crimes to which male offenders are addicted. But there are also cultural factors involved. The seclusion of women, their greater concern with domestic affairs, and their protection from the harsh competition of commercial life, are all influences making against criminal proclivities and opportunities for women. There is, moreover, the likelihood that the courts are more lenient with women offenders, acquitting them out of a false sense of chivalry, or placing them on probation if convicted. For the view that women are inherently less disposed to crime than men there is little evidence.

Besides Wood and Waite, eight other textbook authors propose that physical differences between the sexes are a contributing factor to differential crime rates. (This is shown in Table 1.) One example is Gault's view that the lower crime rate of women "is due to their lack of physical strength and prowess and to lack of opportunity afforded by their position in the economic, industrial, and commercial world, than to any other cause" (Gault, 1932:208–209). While Gault assigns a major role to physical differences, other authors grant only minor importance to the role of physical factors (Wood and Waite, 1941; Taft, 1942, 1950; Barnes and Teeters, 1943, 1959; Caldwell, 1965; Johnson, 1964). More significant, perhaps, is that these authors do *not* treat physical differences between the sexes in terms of genetic or biological determinism—e.g., as if there were a relation between physique, temperament, and crime. The physical factors were seen as setting limits on possible behaviors, specifically that strength, stamina, speed, and so on are important contingencies for certain kinds of criminal activities. The limits could be real or putative in that they reflect selective experience and cultural determination.

The position that biological or physical differences *inherently* lead to sex differences in crime was held only by Reckless (1940, 1950, 1961) and to some extent by Parmelee (1918). According to Reckless:

Biologically and psychologically human maleness may mean greater activity, greater

risk taking, greater aggressiveness, greater wandering; and human femaleness may mean greater inactivity and greater passivity (1950:61).

Note, also, that Reckless utilizes the sociocultural point of view, stating that

Males are accorded by custom greater opportunities and latitudes for movement and activity, and females are more restrained and curtailed in their activity according to custom (1950:61).

None of the authors explain the lower crime rate of women in terms of the idea of innate moral superiority of women. Elliott comes closest to this view, perhaps, when she suggests that women are more honest, trusting, and concerned about others. (Thus, Wilson and Rigsby conclude that Elliott is a supporter of the moral superiority argument.) Elliott is quite clear, however, in linking these moral qualities to domestic roles and especially the socialization of women for the childbearing and mother role. In fact, similar to other textbook writers (e.g., Wood and Waite (1941), Barnes and Teeters, 1943), Elliott explicitly rejects the moral superiority position, stating that "we have no reason for believing that women are inherently 'better' and therefore less criminal." The chief explanation for the disparity in crime rates of men and women seems rather to lie in the special characteristics of their respective cultures ... "The situations in which women find themselves apparently are not so conducive to crimes as are the situations men face. Despite the so-called emancipation of women, the average woman spends her life and fulfills her purposes in the home" (Elliott, 1952:201).

Finally, nine of the authors noted that *officially* reported statistics on crime are likely biased toward underestimating female more than male criminality. (See *s in Table 1.) The emphasis here is on the chivalrous treatment of women by control agents and on the "masked" character of female crime more generally.

AGE VS. SEX DIFFERENCES

Table 1 also shows the amount of space or number of pages devoted to a treatment of age differences in crime. When compared to pages devoted to sex differences, we note that (a) those authors who discuss sex differences also discuss age differences and (b) that the number of pages devoted to age differences is nearly identical to that devoted to sex differences. This comparison does not support the claim of a sex bias in the literature; at least, sex differences in criminality have been no more ignored as a topic "worthy of consideration" than have age differences.

Most authors conclude that age differences in crime are due in part to variations in physical strength, agility, and stamina of youth. Specific mention is made of greater strength and ability of youth (Sutherland, 1934; Johnson, 1964), of higher activity levels, aggressiveness and recklessness of youth (Caldwell, 1956; Taft, 1942), and of stronger sex drives of youth (Taft, 1942; Gillin, 1945). In general, physical factors are used in a parallel fashion by these authors in their efforts to explain age and sex differences in crime. The physical factors are seen as setting limits on possible behaviors and of opening or closing off opportunities for criminal activities. Of further interest, perhaps, some textbook writers (e.g., Sutherland) utilize physical variables to explain age differences in crime but do not include them in an explanation of sex differences in crime.

CONCLUSION

The data for this study were presented in a comparatively simple, straightforward manner and were drawn from all criminology textbooks published from 1918 to 1965. We conclude that the claims of some contemporary analysts of a biological and sexist bias in traditional writings on female criminality are not supported by our survey of criminology textbooks. Since at least the early part of this century when criminology became a subfield of sociology rather than of the medical-legal profession, the rule among American authors of criminology textbooks has been to provide a sociogenic interpretation of sex differences in crime and to avoid or reject

biological explanations and the like. In this respect, the authors anticipate contemporary views on sex differences in crime in which structural and cultural variables are emphasized and biological and psychological factors receive scant attention. This review, and the passages cited, demonstrate more continuity in the developmental processes involved in criminological thought than is sometimes suggested in contemporary writings.

A *caveat* is important here. The findings of this study apply only to criminology textbooks. The legacy of prominent, specialized works on female criminality have not been examined. Nevertheless, to the extent that textbook material is generally limited to those concepts, theories, and findings that are widely accepted within the field (Kuhn, 1962), our findings can be taken as a principal measure of the collective judgment of criminologists on the issue of sex differences in crime. In this regard, the fact that textbooks are aimed at students often obscures the profession itself as a relevant audience. Textbooks are evaluated, accepted, or rejected by members of the discipline before they reach the student (see, e.g., Rothman, 1971). In this way, members of the discipline or a subfield act as "gatekeepers" for theoretical perspectives and types of knowledge incorporated into textbooks. Our review of criminology texts suggests, therefore, the preponderance of the *sociological* paradigm among criminologists and instructors of criminology courses. This is made even more obvious when it is observed that by far the most popular criminology text, that of Sutherland, was perhaps the most antagonistic towards biological explanations of crime and most forceful in acceptance of sociogenic interpretations.

REFERENCES

Adler, Freda
1975 Sisters in Crime. New York: McGraw-Hill.
Barnes, Harry and Negley Teeters
1943 New Horizons in Criminology. New York: Prentice-Hall.
1951 New Horizons in Criminology (2nd ed.). New York: Prentice-Hall.
1959 New Horizons in Criminology (3rd ed.). New York: Prentice-Hall.

Bloch, Herbert
1962 Man, Crime, and Society. New York: Random House.
Bowker, Lee
1978 Women, Crime, and the Criminal Justice System. Lexington, MA: D. C. Heath.
Caldwell, Robert
1956 Criminology. New York: Ronald Press.
1965 Criminology (2nd ed.). New York: Ronald Press.
Cantor, Nathaniel
1939 Crime and Society: An Introduction to Criminology. New York: Henry Holt & Co.
Cavan, Ruth Shonle
1948 Criminology. New York: Thomas Crowell.
1962 Criminology (3rd ed.). New York: Thomas Crowell.
Elliott, Mabel
1952 Crime in a Modern Society. New York: Harper & Brothers.
Ellis, Havelock
1914 Man and Woman. London: Routledge.
Ettinger, Clayton
1932 The Problem of Crime. New York: Harper & Brothers.
Gault, Robert
1931 Criminology. Boston: D.C. Health
Gibbons, Don
1974 "Say, whatever became of Maurice Parmelee, anyway?" The Sociological Quarterly 15:405–416.
1979 The Criminological Enterprise: Theories and Perspectives. Englewood Cliffs, NJ: Prentice-Hall.
Gillin, John
1926 Criminology and Penology. New York: The Century Co.
1945 Criminology and Penology (3rd ed.). New York: Appleton-Century.
Haynes, Fred
1935 Criminology. New York: McGraw-Hill.
Harris, Anthony
1977 "Sex and theories of deviance: Toward a functional theory of deviant type-scripts." American Sociological Review 42:3–15.
Hartung, Frank
1965 Crime, Law, and Society. Detroit, MI: Wayne State University Press.
Inkeles, Alex
1964 What is Sociology? Englewood Cliffs, NJ: Prentice-Hall.
Johnson, Elmer
1964 Crime, Correction, and Society. Homewood, IL: Dorsey.
Kando, Thomas
1978 Sexual Behavior and Family Life in Transition. New York: Elsevier.
Klein, Dorie
1973 "The etiology of female crime: A review of the literature." Issues in Criminology 8:3–30.
Korn, Richard and Lloyd McCorkle
1959 Criminology and Penology. New York: Holt, Dryden.
Kuhn, Thomas
1962 The Structure of Scientific Revolutions. Chicago: University of Chicago Press.

Mannheim, Hermann
1965 Comparative Criminology. Boston, MA: Houghton Mifflin.

Millman, Marcia
1975 "She did it all for love: A feminist view of the sociology of deviance." Pp. 251–279 in Marcia Millman and Rosabeth Moss Kanter (eds.), Another Voice. Garden City, NJ: Anchor.

Mills, C. Wright
1943 "The professional ideology of social pathologists." American Journal of Sociology 49:165–180.

Morris, Albert
1934 Criminology. New York: Longmans, Green.

Parmelee, Maurice
1918 Criminology. New York: Macmillan.

Parsons, Philip
1926 Crime and the Criminal. New York: Alfred Knopf.

Pollak, Otto
1950 The Criminality of Women. Philadelphia: University of Pennsylvania Press.

Rasche, Christine
1974 "The female offender as an object of criminological research." Criminal Justice and Behavior 1:301–320.

Reckless, Walter
1940 Criminal Behavior. New York: McGraw-Hill.
1950 The Crime Problem. New York: Appleton-Century.
1961 The Crime Problem (3rd ed.). New York: Appleton-Century.

Rothman, Robert
1971 "Textbooks and the certification of knowledge." The American Sociologist 6:125–127.

Schlapp, Max
1928 The New Criminology. New York: Boni & Liveright.

Simon, Rita James
1975 The Contemporary Woman and Crime. National Institute of Mental Health. Washington, DC: U. S. Government Printing Office.

Smart, Carol
1976 Woman, Crime and Criminology: A Feminist Critique. London: Routledge, Kegan Paul.

Sutherland, Edwin
1924 Criminology. Philadelphia: J. B. Lippincott.
1934 Principles of Criminology (2nd ed.). Philadelphia: J. B. Lippincott.
1947 Principles of Criminology (4th ed.). Philadelphia: Lippincott.

Sutherland, Edwin and Donald Cressey
1960 Criminology (6th ed.). Philadelphia: Lippincott.

Taft, Donald
1942 Criminology. New York: Macmillan.
1950 Criminology (rev. ed.). New York: Macmillan.

Taft, Donald and Ralph England
1964 Criminology (4th ed.). New York: Macmillan.

Tannebaum, Frank
1938 Crime and the Community. Boston: Ginn & Co.

Tappan, Paul
1960 Crime, Justice and Correction. New York: McGraw-Hill.

Theodorson, George and Achilles Theodorson
1969 Modern Dictionary of Sociology. New York: Thomas Crowell.

Thomas, W. I.
1907 Sex and Society. Boston: Little, Brown and Company.

Weir, Elligius
1941 Criminology: A Scientific Study of the Modern Crime Problem. Joliet, IL: Institute for Scientific Study of Crime.

Weitz, Shirley
1977 Sex Roles: Biological, Psychological, and Social Foundations. New York: Oxford University Press.

Wilson, Nanci and Constance Rigsby
1975 "Is crime a man's world? Issues in the exploration of criminality." Journal of Criminal Justice 3:131–140.

Wood, Arthur and John Waite
1941 Crime and Its Treatment. New York: American Book Co.

SPECIAL READERS
August 1979 to August 1980

Freda Adler
Elliot Aronson
William Bainbridge
Paul J. Baker
Jeanne Ballantine
Mary Jo Bane
Bernard Barber
Alan Bates
Alan Bayer
George Becker
Stith Bennett
Nijole Benokraitis
Ilene Bernstein
Kenneth Berry
Albert Biderman
Peter Bishop
Hubert Blalock
Herbert Blumer
Rhoda Blumberg
Philip Blumstein
Edna Bonacich
Edgar Borgatta
Phillip Bosserman
Lee Bowker
Ted Bradshaw
John Michael Brooks
Julia Brown
Brent Bruton
Marguerite Bryan
Roy Bryce-Laporte
Neil Bull
Michael Burawoy
Thomas Burger
Leonard Cain
Theodore Caplow
Maren Carden
Albert Chabot
Burton Clark
Alfred Clarke, Jr.
Frank Clemente
Stephen Cole
Randall Collins
Jay Corzine
Rose Coser
Herbert Costner
Diana Crane
Kathleen Crittendon
Mihaly Csikszentmihalyi
William D'Antonio
Dwight Dean
Lois DeFleur
Melvin DeFleur
Kevin Delinger
Irwin Deutscher
Timothy Diamond
Bonnie Dill
Dean Dorn
Richard Dukes
Eric Dunning
Douglas Eckberg
Harry Edwards
Howard Ehrlich
Thomas Fararo

Robert Faulkner
Joseph Fichter
Stephen Fitzgerald
William Form
Roy Francis
Arthur Frank, III
Jonathon Freedman
Morris Fried
Robert Friedrichs
John Galliher
Zelda Gamson
Herbert Gans
Blanche Geer
Reed Geerston
Janet Giele
Sheryl Gilkes
Derek Gill
Barney Glaser
Barry Glassner
Tom Good
Margaret Gordon
Gerald Grant
Bradford Gray
David Gray
Andrew Greeley
Penelope Green
Vaughn Grisham
Edward Gross
William Gum
Dee Spencer Hall
Eleanor Hall
Richard Hall
Fred Halley
Kelley Hancock
Lowell Hargens
John Hartman
Philip Hauser
Ronald Havelock
Darnell Hawkins
Susan Heckman
Jack Hedblom
James Henslin
Robert Herriott
Richard Hessler
Joseph Himes
Gisela Hinkle
Travis Hirschi
Daryl Hobbs
Paul Horton
George Huaco
Helga Huang
Joan Huber
Everett Hughes
Laud Humphreys
Mamoru Iga
Alison Jaggar
C. R. Jeffery
Robert Alun Jones
Gay Kang
Tai Kang
Geoffrey Kapenzi
Roy Kaplan
David Kaufman

Theodore Kemper
Charles Key
Lewis Killian
John Kitsuse
Frank Kohout
John Kunkel
Otto Larson
Robert Leik
Charles Lemert
Adeline Levine
Donald Levine
Nan Lin
Charles Lindblom
Lora Liss
Craig Little
John Lofland
Guenther Lueschen
Gene Lutz
Stanford Lyman
John Maiolo
Michael Malec
Ron Manuel
Katherine Marconi
Donald Martindale
Rosanne Martorella
Betty Maynard
William Mayrl
Allan Mazur
Bernard Meltzer
Theodore Mills
Kent Mommsen
Joan Moore
Omar Moore
Wilbert Moore
Nicholas Mullins
Reece McGee
Robert McGinnis
Scott McNall
Sharon McPherron
Krishnan Namboodiri
Zvi Namenwirth
Mary Jo Neitz
Roger Nett
Peter New
Richard Ogles
Marvin Olsen
Ronald Pavalko
Robert Perrucci
Caroline Persell
James Petras
Derek Phillips
James Pinkerton
Ithiel Pool
Elwin Powell
Suzanne Powers
Richard Quinney
Sal Restivo
Lawrence Rhoades
Peter Richerson
David Riesman
Kathleen Ritter
Paul Ritterband
George Ritzer

Alice Rossi
Ino Rossi
Guenther Roth
David Sallach
Leonard Schatzman
Jackie Scheer
Trent Schroyer
Michael Schudson
Clarence Schultz
Richard Schwartz
Herman Schwendinger
Robert A. Scott
David Segal
Lauren Seiler
Robert Sheets
Elbridge Sibley
Gideon Sjoberg
Dorothy Smith
Pat Smith
William Snizek
Warren Solomon
Eve Spangler
Nico Stehr

Nancy Stein
Patricia Steinhoff
Arthur Stinchcombe
Brad Stone
Norman Storer
Anselm Strauss
David Street
Sheldon Stryker
Ann Sundgren
Seymour Sudman
William Swatos, Jr.
Dalmos Taylor
Warren TenHouten
Athena Theodore
Michael Thorn
Charles Tittle
Gaye Tuchman
Jonathon Turner
Ralph Turner
Andrew Twaddle
Herbert Ulrich
Michael Useem
Pierre van den Berghe

Edwin Vaughan
Theodore Wagenaar
Andrew Walker
Ruth Wallace
Walter Wallace
Richard Warnecke
Maria-Barbara Watson-Franke
Murray Wax
Rosalie Wax
Eugene Weinstein
Jay Weinstein
Kenneth Westhues
Arthur Wilke
Doris Wilkinson
Charles Willey
Sidney Willhelm
Robin Williams
Everett Wilson
John Wilson
Kurt Wolff
Gail Woodstock
Richard Zaner
Maurice Zeitlin

MANUSCRIPTS FOR THE
ASA ROSE SOCIOLOGY SERIES

Manuscripts (100 to 300 typed pages) are solicited for publication in the *ASA Arnold and Caroline Rose Monograph Series*. The Series welcomes a variety of types of sociological work—qualitative or quantitative empirical studies, and theoretical or methodological treatises. An author should submit three copies of a manuscript for consideration to the Series Editor, Professor Suzanne Keller, Department of Sociology, Princeton University, Princeton, NJ 08540.

Criminal Behavior
Readings in Criminology
edited by DELOS H. KELLY, *California State University, Los Angeles*

Criminal Behavior introduces the student to criminology through an appealing mix of theoretical essays and descriptive accounts of criminal careers. The articles illustrate how various types of crimes and criminal careers evolve, are maintained, and may be transformed.

August 1980   paperbound

Single Life
Unmarried Adults in Social Context
edited by PETER J. STEIN, *William Paterson College*

Single Life is the first comprehensive collection of essays dealing with the diverse population of single adults—a large, growing, and often misunderstood segment of today's society. The selections examine the life styles and major life issues of the divorced, separated, and widowed, as well as the never married.

February 1981   paperbound

The Sociology of Health and Illness
Critical Perspectives
edited by PETER CONRAD, *Brandeis University*, and ROCHELLE KERN, *New York University*

Organized around critical issues in the sociology of health and illness, these readings represent a variety of perspectives. Together, the selections explore the social roots and manifestations of the health care crisis in American society.

February 1981   paperbound

It's A Living
Work in Modern Society
GALE MILLER, *Marquette University*

It's A Living looks at the ways in which people make their livings and give meaning to their work. The text examines a wide range of activities, including professional and factory work, peasant labor, housework, and work in alternative communities.

February 1981   paperbound

To request a complimentary examination copy, please write
(specifying course, present text, and approximate enrollment) to:
■**St. Martin's Press • P.O. Box 5352 • New York, N.Y. 10163**■

JHSB publishes reports of empirical studies, theoretical analyses, and synthesizing reviews that employ a sociological perspective to clarify aspects of social life bearing on human health and illness, both physical and mental. Its scope includes studies of the organizations, institutions, and occupations devoted to health services, as well as studies of the behavior of actual and potential recipients of these services.

Recent issues contain reports on:

Social Class and the Use of Professional Help for Personal Problems: 1957 and 1976, RICHARD A. KULKA, JOSEPH VEROFF & ELIZABETH DOUVAN

Health Occupations in Israel: Comparative Patterns of Change During Socialization, JUDITH T. SHUVAL AND ISRAEL ADLER

Occupational Stress and Health Among Factory Workers, JAMES S. HOUSE, ANTHONY J. McMICHAEL, JAMES A. WELLS, BERTON H. KAPLAN, & LAWRENCE R. LANDERMAN

The Functional Importance of Sex Roles and Sex-Related Behavior in Societal Reactions to Residual Deviants, WILLIAM A. RUSHING

Stress, Social Status, and Psychological Distress, RONALD C. KESSLER

ROSE MONOGRAPH SERIES

(Published by Cambridge University Press)

(Suzanne Keller, Editor)

The ASA Rose Monograph Series, established in 1968 by a fund set up by Arnold and Caroline Rose, was developed to publish high quality monographs and theoretical studies in sociology. Titles include:

Middle Start: An Experiment in the Educational Enrichment of Young Adolescents
(Yinger, Ikeda, Laycock, Cutler)   (Cloth—$13.95; Paper—$4.95)*

Education, Employment and Migration: Israel in Comparative Perspective
(Ritterband)   (Cloth—$13.95; Paper—$4.95)*

Class, Race and Worker Insurgency: The League of Revolutionary Black Workers
(Geschwender)   (Cloth—$13.95; Paper—$4.95)*

Protest and Participation: The New Working Class in Italy
(Low-Beer)   (Cloth—$16.95; Paper—$4.95)*

Continuity and Change: A Study of Two Ethnic Communities in Israel
(Simon)   (Cloth—$13.95; Paper—$4.95)*

Cities with Little Crime: The Case of Switzerland
(Clinard)   (Cloth—$13.50; Paper—$4.95)*

Opening and Closing: Strategies of Information Adaptation in Society
(Klapp)   (Cloth—$13.95; Paper—$4.95)*

Tasks and Social Relationships in Classrooms: A Study of Instructional Organization and Its Consequences
(Bossert)   (Cloth—$14.95; Paper—$4.95)*

Juvenile Delinquency and Its Origins: An Integrated Theoretical Approach
(Johnson)   (Cloth—$14.95; Paper—$4.95)*

Understanding Events: Affect and the Construction of Social Action
(Heise)   (Cloth—$14.95; Paper—$4.95)*

From Student to Nurse: A Longitudinal Study of Socialization
(Simpson)   (Cloth—$14.95; Paper—$4.95)*

Order the above monographs from: CAMBRIDGE UNIVERSITY PRESS, 32 East 57th Street, New York, New York 10022

Books previously published in the series ... Available only in paperback at $5.00 each from the ASA, 1722 N Street, N.W., Washington, D.C. 20036:

Socioeconomic Background and Educational Performance (Hauser) • *Black and White Self-Esteem: The Urban School Child* (Rosenberg & Simmons) • *Looking Ahead: Self-Conceptions, Race and Family as Determinants of Adolescent Orientation to Achievement* (Gordon) • *Black Students in Protest: A Study of the Origins of the Black Student Movement* (Orum) • *Attitudes and Facilitation in the Attainment of Status* (Gasson, Haller & Sewell) • *Patterns of Contact with Relatives* (Klatzky) • *Interorganizational Activation in Urban Communities: Deducations from the Concept of System* (Turk) • *The Study of Political Commitment* (DeLamater) • *Ambition and Attainment: A Study of Four Samples of American Boys* (Kerckhoff) • *The Greek Peasant* (McNall) • *Patterns of Scientific Research: A Comparative Analysis of Research in Three Scientific Fields* (Hargens) • *Ethnic Stratification in Peninsular Malaysia* (Hirschman) • *Deviance, Selves and Others* (Schwartz & Stryker)

*Individual members of the A.S.A. are entitled to a special discount. Contact:

AMERICAN SOCIOLOGICAL ASSOCIATION
1722 N Street, N.W.
Washington, D.C. 20036

Sociology of Education publishes papers on educational processes, and on human development. The research may focus on the individual, institutions, and structural arrangements among institutions bearing on education and human development.

Recent issues have included studies on:

Interactive Influences in Spady and Tinto's Conceptual Models of College Attrition—PASCARELLA & TERENZINI

Loose Structure, Retrenchment and Adaptability in the University—RUBIN

School SES Influences—Composition or Context?—ALEXANDER, FENNESSEY, McDILL & D'AMICO

Academic Sponsorship and Scientists' Careers—RESKIN

Three Perspectives on IQ and Social Differentiation—GUTERMAN, GORDON & RUDERT, ECKLAND

EDITOR'S PAGE

would question sharply if they came from a novice."

On the other hand, an unknown and probably young author objects: "What chance do I have if I have to compete openly with the big names in my field? Blindness reduces their advantage and makes competition fairer." Many editors are certain that, if refereeing is open, opinions will not be frank, for fear of retaliation. An editor declared, "I insist that board members sign their recommendations and defend them if challenged. But what can I do when reviewers beg me to keep their names secret to protect them from the authors' pettiness, or malice?" At this a fellow-editor confessed, "I have yet to see convincing proof of the superiority of *any* system." Like the secret ballot, blind reviewing can save the situation when colleagues are asked to pass judgment on work authored by friends, or departmental chairs, or former teachers. I have not heard of a journal which, having gone over to blindness after years of open refereeing ever turned back again. The new Marxist journals and journals animated by ideologies of humanism tend to stand for openness: "We are not elitist," they say.

The debate on the merits of the two systems of peer review will not come to a conclusion as long as there is no consensus on the criteria by which papers are judged. For one thing, the ordinary scholar sees only the papers that are accepted. But what of the 80 or 85 out of every 100 submissions which are not accepted? Where are the old file cards? If we should collect all the file cards of the three generalist journals for two or three years we might discover how openness or blindness actually enter into peer reviewing. The records of the rejected might bring to light some interesting unsuspected preferences and tabus.

At its best, peer review can be an invigorating collegial experience in which authors, editors, and reviewers share their opinions and their knowledge of a given field, and learn from each other, as in a seminar by correspondence. Their collaboration can vastly improve scholarly reporting. But there is a dark side of peer reviewing. Although, judging from their published statements, journal editors avoid discussing its seamy side, there certainly is one, as in every occupation, and among themselves they muse over ethical problems with terrible candor.

Thus, at the last convention of the Society for Scholarly Publishing, in a session called "Ethical Questions of the Peer Review System," the editor-in-chief of the *Journal of Bacteriology* asked his audience of editors about difficulties arising from unreasonable harshness and excessive finding of faults; about reviewers who delay a manuscript so they can rush into print with their own competing paper; about reviewers who incorporate ideas from a manuscript under review in their own current paper, or, more often, in their work in progress; about the ethics of using ideas prior to publication. His conclusion is that these usages are real, but rare, and that peer reviewing cannot be maintained without anonymity (Silver, 1979). Walter Gove (1979: 802) tells of authors who invent false versions of research—even of their own—or publish reports of contradictory findings, as though each were valid. The temptation to engage in unethical behavior may arise when publication is sought primarily to promote a career, rather than the understanding of human society. It is time now for a thorough enquiry into scholarly ethics. How much unethical behavior is there? How seriously are colleagues hurting each other?

REFERENCES

Crane, Diana
    1967   "The gatekeepers of science: Some factors affecting the selection of articles for scientific journals." The American Sociologist 2:195–201.

Gove, Walter R.
    1979   "The review process and its consequences in the major sociology journals." Contemporary Sociology 8:799–804.

Silver, Simon
    1979   "Ethical questions in the peer review system." Paper read at the meeting of the Society for Scholarly Publishing, Boston, June 5.

Journals of the
American Sociological Association

3 0 MAR 1981

AMERICAN SOCIOLOGICAL REVIEW
(Rita J. Simon, *Editor*)   Bi-monthly—ISSN 0003-1224)

Official Journal of the A.S.A. Articles of major concern to social scientists. New trends and developments in theory and research.
- Members, $10   Non-members, $15   Institutions, $30
          Non-member students, $10   Single issues, $4

CONTEMPORARY SOCIOLOGY: A Journal of Reviews
(Norval D. Glenn, *Editor*)   Bi-monthly—ISSN 0094-3061)

A journal of reviews, criticism, and debate designed to give new thrust and style to reviewing and intellectual exchange within the field.
          Members, $10   Non-members, $15   Institutions, $20
          Non-member students, $10   Single issues, $4

JOURNAL OF HEALTH & SOCIAL BEHAVIOR
(Howard B. Kaplan, *Editor*)   Quarterly—ISSN 0022-1465)

Distinctive for a sociological approach to the definition and analysis of problems bearing on human health and illness.
          Members, $8   Non-members, $12   Institutions, $16   Single issues, $4

SOCIOLOGY OF EDUCATION
(Alan C. Kerckhoff, *Editor*)   Quarterly—ISSN 0038-0407)

A forum for social scientists concerned with research on education and human social development.
          Members, $8   Non-members, $12   Institutions, $16   Single issues, $4

SOCIAL PSYCHOLOGY QUARTERLY
(George W. Bohrnstedt, *Editor*)   Quarterly—ISSN 0190-2725)

A journal of research and theory in social psychology. Genuinely interdisciplinary in the publication of works by both sociologists and psychologists.
          Members, $8   Non-members, $12   Institutions, $16   Single issues, $4

THE AMERICAN SOCIOLOGIST
(James L. McCartney, *Editor*)   Quarterly—ISSN 0003-1232)

Contains major articles analyzing sociology as a profession and as a discipline. Included are reports on standards and practices in teaching, research, publication and the application of sociological knowledge.
          Members, $8   Non-members, $12   Institutions, $16   Single issues, $4

AMERICAN SOCIOLOGICAL ASSOCIATION
1722 N Street, N.W. • Washington, D.C. 20036